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## THE FRANKLIN INSTITUTE.

*Stated Meeting, held Wednesday, February 19, 1902.*

President JOHN BIRKINBINE in the chair.

## THE METRIC SYSTEM OF WEIGHTS AND MEASURES.

### DISCUSSION.

THE CHAIRMAN:—There is now in order a discussion of the Report of the Special Committee on the Metric System. That is not a new subject before the Franklin Institute, nor wanting in interest to a body devoted to the mechanical arts. It is a subject which the Institute has taken cognizance of a number of times, and of which we must now take cognizance, remembering that any statement we make, as the Franklin Institute, goes forth with whatever influence we have behind it, for that purpose. When the matter was presented at a meeting several months ago, it was referred to a committee selected with care and with the view of having all the Sections represented. The committee has worked well and made a report which you have all seen and which is here for your approval or disapproval.

Supplementing the work of the committee, the Secretary addressed a number of letters to parties—members of the Institute and others, whose opinions on the subject it was thought desirable to obtain. Whether the Institute is prepared to vote on the matter to-night is a matter for you to decide, but it is certainly desirable to discuss the subject as thoroughly as we can. The report will now be read by the Secretary.

REPORT OF THE SPECIAL COMMITTEE APPOINTED AT THE STATED MEETING OF NOVEMBER 20, 1901, TO REPORT ON THE FEASIBILITY AND ADVISABILITY OF THE ADOPTION OF THE METRIC SYSTEM OF WEIGHTS AND MEASURES IN THE UNITED STATES.

WHEREAS, It is desirable to obtain an international standard of weights and measures, also to simplify and regulate some of our existing standards; and

WHEREAS, The metric system is commendable not only as a suitable international standard, but also for facility of computation, convenience in memorizing and simplicity of enumeration;

*Resolved*, That the Franklin Institute approves of any movement which will promote the universal introduction of the metric system with the least confusion and expense.

*Resolved*, That the National Government should enact such laws as will ensure the adoption of the metric system of weights and measures as the sole standard in its various Departments as rapidly as may be consistent with the public service.

JAMES CHRISTIE,
American Bridge Company, Pencoyd, Pa.

A. E. KENNELLY,
Houston & Kennelly, Electrical Engineers.

F. E. IVES,
Photographer and Engraver.

WILFRED LEWIS,
President Tabor Machine Company.

S. M. VAUCLAIN,
Superintendent Baldwin Locomotive Works.

JESSE PAWLING, JR.,
Instructor in Physics, Central High School.

GEORGE F. STRADLING,
Instructor in Physics, N. E. Manual Training School.

HARRY F. KELLER,
Professor of Chemistry, Central High School.

A. FALKENAU,
Mechanical Engineer and Machinist.

L. F. RONDINELLA,
Instructor in Engineering, Central Manual Training School.

APPENDIX.

*Questions discussed at meeting of Sub-Committee, January 17, 1902.*

| Questions. | Answers Agreed upon. |
|---|---|
| (1) Assuming the desirability of an international standard, could we expect nations using the metric system to abandon that and adopt our system? | No. |
| (2) Can we not concede the advantages of the metric system for purposes of computation, and also as being readily memorized and the relations between weights and measures borne in mind without much effort. | Yes. |
| (3) Have any valid objections against the metric system been effectively urged, excepting that the numeration cannot be continuously subdivided by two? | No. |
| (4) Is not this similar objection to our decimal currency overcome by the advantages of the system? | Yes. |
| (5) For convenient minimum units of hand rules, is not the mm. better than either $\frac{1}{64}''$ or $\frac{1}{32}''$; the latter being rather a fine subdivision for ordinary rough measurements? | The mm. is equally as convenient. |
| (6) Assuming that the change in our system could be effected, without serious expense or confusion, could we recommend this change as desirable? | Yes. |
| (7) Could not such a change be fairly initiated if the National Government would adopt the system in all its Departments, where no serious confusion would occur from an early change, gradually extending the system to other Departments, when people became accustomed to its use, and tools were accumulated which conformed to the new standard? | Yes. |
| (8) In the workshops, could not a large proportion of existing tools and gauges be retained until they were gradually superseded, merely designating their nominal dimensions in the nearest convenient metric units? | We anticipate no prolonged serious confusion. |
| (9) If in the course of a term of years, the system came into universal use in the service of the Government, is it probable that its adoption would follow elsewhere within a reasonable time? | Yes. |
| (10) Would it appear to be practicable to inaugurate the adoption of the metric standards for weights or for liquid measures, in advance of linear measures, as the former would not involve the abandonment of such numerous and costly tools as would the latter? | No. |

THE CHAIRMAN:—I shall ask Mr. George M. Bond, of the Pratt & Whitney Company, of Hartford, Conn., to open the discussion.

MR. BOND:—It is with a good deal of diffidence that I undertake to make the remarks that are called for, as I had expected to hear the discussion in advance, so that I could have something to refer to that would be, perhaps, to the point; but I would say, in a preliminary way, that it was only lately that I attended a hearing before the Committee on Coinage, Weights and Measures, in Washington. I was somewhat surprised to find how few were present at the meeting, which I presumed was to afford the representative manufacturers of the country the opportunity of expressing their views on the suitableness or the practicability of introducing compulsory measures looking toward the adoption of the metric system. I stated to the Committee at the time that while I was fully aware of the advantages of the metric system in its application for computation and scientific purposes, I was not aware of its great convenience in ordinary workshop practice. I have had some experience in linear work, not in weights or in liquid or dry measures; so that I am not prepared to say anything in regard to either of the latter at this time; but I told the Committee that I considered the English units, such as are adopted and have been used for years—the inch, and yard, and foot—as tending more to convenience and to economy than would be the case with the subdivisions or the units as represented by the millimeter or the meter. The millimeter is too small for the purposes of the workshop, in that it has to be multiplied many times if decimals are to be avoided and if all the other subdivisions of the meter are to be avoided. If the meter is the unit, then, of course, the decimal point would have to be used.

It is a serious matter to all concerned that the meter was adopted of the length that it was, in terms of the quadrant of the earth's surface, which we all know was a mismeasurement; but if it had been correctly determined, or better still, if it had been made commensurate with the inch, so as to be 40 inches long, then this change which is now advocated would be a comparatively simple matter where the translation has to be carried from one to the other. We all know that it is impossible for certain kinds of work, without carrying out the relation to a number of decimals, to make this interchange or translation within the limits of necessary accuracy.

It has also occurred to me at this time, and at this meeting, to ask if it had been considered what changes are going to be required in the Army and Navy Departments. That clause in the House of Representatives' bill covers the almost immediate adoption of the metric system by the Government in all its Departments. The Navy Department has, as we all know, adopted the Franklin Institute or Sellers thread for bolts and nuts. That system enters into all construction by the Navy Depart-

ment and has been used for a great many years. It was on the recommendation of the Franklin Institute that this thread was adopted by the Department, and the change to the metric system for the Navy Department in that one item alone would be a serious matter. It would be almost impossible to make the change in even units so as to make the work as interchangeable as it is now required. Another thing, the railroads of the country have adopted the United States standard thread, and it is now in use on every railroad of the country; and it is really the officially recognized standard of all railroads by the action of the Master Car Builders' and the Railway Master Mechanics' Associations.

These standards are all in English measure, and the pitches are English; so that there would have to be a translation, which means a good deal of trouble and expense. Not only drawings must be made in the metric system, but also all specifications. It is not possible to make these changes in the units without changing standards that have been in service a great many years. I understand that at the next hearing of the Committee at Washington representatives of the Army and Navy Department will be called in, and perhaps a number of manufacturers representing the largest industries of the country. It will be of a good deal of interest to know what will be their views on the subject. I can only speak for our own company, of course; and I know that this change is deprecated, and we would hesitate a long time before we would undertake to make it. Of course, if made compulsory by law it would be a necessary measure. The law as already passed by Congress legalizes the use of the metric system, and that, of course, is fair to those whose business requires the use of it and who find it more convenient; it is perfectly right that they should use it; and I have long held and expressed the opinion that the metric system has its advantages and I appreciate them; that they are specially adapted for computation, for calculation; but I do not think, from the experience that we have had and that I have had, especially in certain lines of work, that it would be of the great advantage that is claimed for it.

I have not seen the report or any account of the testimony of representatives before the Committee on Coinage, Weights and Measures, nor do I know the manufacturers who have appeared. I understood at the time of my attendance that it was to be a meeting called for manufacturers, and I was surprised to find myself the only one, except one gentleman who represented a very important industry—that of scientific and optical instruments of the very highest order; and I appreciate and I fully understand that the metric system in his work is absolutely necessary for the reason that his work is entirely with the scientific public, and, of course, scientific persons find the use of the metric system of advantage. That goes without saying; we do not argue that case at all; but to have the law framed as the bill certainly

reads at this time (there is no other interpretation of it, so far as I can judge), means the compulsory adoption of the metric system after a certain date. It is very specifically stated so for all the Departments of the Government. That is the point I want to bear upon more particularly to-night, for the reason that most large manufacturing concerns (our own included) have a great deal of business with the Government, and we must know whether it will be necessary that our contracts be specified in the metric system—all machines, drawings, and all of the specifications made in the metric system after a certain date. That, of course, will be a ruling which will be necessary if the bill is passed, and if this had to be done it would mean considerable expense and trouble.

As far as concerns the clause relating to compulsory use otherwise than in the Government Departments, that is not so clearly stated; but it implies compulsory adoption in about two and a half years. That is the way it is interpreted by persons who are quite familiar with the text as represented by the bill, and it certainly will not be thus accepted and allowed to pass without a protest which will be entered by many manufacturers. It may not be by a majority, but there will be some who will demand to be heard in the matter.

THE CHAIRMAN:—The Chair wishes to apologize to the Chairman of the Committee, Mr. Christie, for not calling first upon him to speak for the report, but under the circumstances as he has been informed that Mr. Bond had to leave by an early train, he felt that the Institute would excuse that omission, and he trusts also that Mr. Christie will do so. Before asking Mr. Christie to continue the discussion, I shall ask Dr. Wahl merely to read the names of those who have contributed to the discussion by correspondence, because it may be that you may wish to call for the reading of some of these letters. After the names have been read I shall give Mr. Christie the floor.

THE SECRETARY:—This correspondence has accumulated very rapidly, and a large portion of it came to my hand only yesterday and to-day, and I have had opportunity only to arrange it into two classes: one favoring the report, and the other either non-committal or unfavorable. [Reads names of correspondents.]

MR. JAMES CHRISTIE:—In presenting for your consideration the report of the Committee on the Metric System, it is perhaps unnecessary for me to assure you that the results of the deliberations of the committee were not derived hastily, or without due thought and consideration being given to the subject.

It is a serious matter to alter our old and well-established systems of weights and measures, and such a change could not be justified unless the advantages to be gained were of such profound importance as to warrant it. We are required to consider the subject in all its bearings and its manifold relations to the needs of every element in the commu-

nity. The fact that our present systems, especially as they apply to weights and measures of capacity, are inconsistent and incongruous, is not alone sufficient reason to entirely abandon them. If this alone was the only objection, we could readily remove the imperfections, whilst retaining the fundamental units, but we are required to consider not only internal economy and convenience, but also international relations with the rest of the world.

The metric system of weights and measures has outlived the century, and a review of its extension and growth indicates that it is gradually but surely superseding the ancient systems throughout the civilized world. The English-speaking people so far have not accepted it in their commercial methods. Nevertheless, we find the system is intrenching itself both here and in Great Britain, and whilst it has not yet had any popular recognition here, it is probably only a question of time before we will realize that we have in existence a dual system of weights and measures.

It will be observed that hitherto the most earnest advocates for the adoption of this system have been found amongst professional or scientific men, or those who are required to make extensive or laborious computations. On the other hand, manufacturers and merchants have either been indifferent or been opposed to a change. The reasons for this are self-evident. The chief advantages of the metric system lie in the correlation existing between its fundamental units. This harmony of relations tends to facilitate computations and largely reduces the strain on the memory in arithmetical calculations. Evidence obtained from those who are constantly required to make extensive computations by both methods, indicates that a large amount of time would be saved for computers if the computations were based upon the metric system instead of our own existing system. It is probably no exaggeration to state that one-half the time of the average computer would be saved in technical and commercial calculations, if made according to metric units. The saving in the time of the computer is not the only economy, but we should consider also the facility with which one can retain in memory the fundamental elements of the system. It is probable that the large majority never attempt to memorize entirely our own system of weights and measures, but keep hand-books convenient for reference. On the other hand, as a personal experience, although I rarely have to use the metric system, when I do apply it I find I have a clearer comprehension of it in my mind than I have with many of our own tables of weights and measures. It is probable that a very considerable proportion of the strain on the school child would be relieved if it dealt with metric units in the study of arithmetic and its applications.

If we had to consider the subject solely from the workshop point of view, for its convenience in linear measurements, etc., I would then say

we need no change at all. I can see no reason why we cannot use our inch or foot just as well as we could use any subdivision of the meter. A disadvantage of any decimal system arises from the fact that it cannot be continuously subdivided in halves; but this is only one point of view, and in all such cases we have to balance advantages against disadvantages. It is frequently urged that the popular mind does not instantly or readily grasp a decimal subdivision. This is largely a habit of thought, and our experience with our decimal currency tends to disprove it. I am sure that the conceptions of 25, 50 or 75 cents are not more readily grasped by the ordinary mind if we designate these sums as ¼, ½ or ¾ of a dollar.

The objections sometimes urged against the metric system that its units are inconvenient for practical application, that the meter is too large and the millimeter too small a unit, are not conceded by those who habitually use this system only. On the contrary, it is claimed that the millimeter is a more convenient minimum unit for the ordinary everyday service than either our one-sixteenth or one-thirty-second of an inch; the former being too large and the latter too small for convenience on hand scales. Any decimal multiple of the minimum unit, varying from the millimeter to the myriameter, can be used as the base unit, according to the magnitude of the measurement involved, and these different units are readily interconvertible by a simple mental process or by the transposition of a decimal point, in marked contrast to the inconvenience found in converting our linear units of inches, feet, yards, fathoms, rods, chains, furlongs, miles, and several other units of length, which are all more or less used in special applications.

Of late years we find many strenuous advocates of the metric system in the ranks of our manufacturers, mechanics and merchants. I think this spread of opinion arises from two causes: first, the great increase in late years of our interchange of products with foreign nations that are now using the metric system, and hence the growing realization of the necessity for an international system of weights and measures. Secondly, the large amount of computation connected with all business enterprises in modern times. The amount of computation that is now made in connection with manufacturing enterprise is much greater in proportion to the product than it was at any former time in the world's history. This is owing to all business being conducted on a more scientific basis than heretofore, and the amount of specialization is vastly increased. Take our steel works, for example; men still in active life recall the time when no chemists were employed in connection with our steel industries, and but few, if any, technically trained engineers. Results were arrived at by shrewd, skilful men, trained to habits of close observation, and the work that was accomplished was wonderful, considering the limited precedents that guided them. These men, like the

architects of old, "builded better than they knew." Now we find our workshops and offices filled with men who have been thoroughly trained in the applied sciences. The work they perform is the result of laborious calculations, from which the elements of chance are eliminated as far as possible. To these men any system of metrology is a great desideratum, which results in a saving of time or needless labor; and in the ranks of these you will find the strongest advocates for the adoption of the metric system.

The strongest objection that is usually raised against such an innovation as herewith proposed, is the confusion and large expense involved in the change. It is true we will have to endure some temporary confusion, but the amount of expense necessarily connected with the change is probably much over-estimated. It is usually assumed that if we are called upon to apply the metric system of measures in our manufacturing establishments, the immediate result would be the replacement of large quantities of tools, especially of gauges and measuring-devices that are made to conform to these gauges. The fact of the matter is, to the workman the unit of measurement, so far as fine subdivisions are concerned, is a nominal distinction. He works from gauges the dimensions of which originate in the tool-room. So far as rough measurements are concerned and where the hand-rule is used, it can make but little difference to the workman what unit of measurement is adopted. He can soon readjust his thoughts and acts to suit any system. If called upon to adopt the metric system in any establishment under my control, I would make no immediate change in any of the tools, but use them as they are, merely denominating them in the nearest convenient metric unit. For example, call an inch 25 millimeters, and so on with any multiple or subdivision of the inch. It would be urged against this that there is no correspondence between the absolute and nominal dimensions of the tool. This, although very desirable, is not absolutely essential; in fact, in many of our tools, the dimensions are only nominal and not exactly real. It would be desirable, however, to begin to make new gauges and tools to replace the old ones as they were worn out. These could be made of actual dimensions to correspond closely with the nominal. The consequence would be that workshops would have for a period two standards, the old and the new; the old applying to all existing products and designs, and the new tools to be applied to new designs and products. The former would gradually disappear and the latter would in time entirely take their place. The manufacturer simply would maintain a double standard—all named in metric units—the one gradually disappearing to be replaced eventually and entirely by the new standard. Many shops at present have two or more standards of dimensions in service, between which they have to preserve distinction, and it would simply require some extra care to prevent confusion or error.

The subject of screw-threads is also frequently quoted and the difficulty and confusion that would result if we were to attempt to change our present system. I may ask, in reply, why change it at all? If it is good, retain it as it is indefinitely. The pitches of screw-threads are entirely arbitrary and the difference could be denominated by the letters of the alphabet or by any other distinction that is desired. The workmen produce these screw-threads by the change gears of their lathes, or by special tools that are furnished them for the purpose, and it is a matter of indifference to them from what unit of measure they are derived. If in the course of time a revision of the system of screw-threads should be desirable, they might then, as a matter of consistency, be based upon metric units; but until such a contingency occurs, there can be no object in making any change.

The opponents of the metric system frequently attempt to stifle it by ridicule. When the Stone Bill was before Congress, a few years ago, a rather humorous speech was made by one of the members that caused considerable amusement, and it was claimed that it had some effect in bringing votes to the opposition. A system, however, that has endured for a century and received as wide acceptance as this, cannot be silenced by an epigram or disposed of with a sneer.

The foreign sound to the names is objected to. If the meter or the kilogram or the liter are not considered euphonious or acceptable, I am sure it is easy to translate them into desirable terms. "A rose by any other name would smell as sweet;" remembering, however, that terms or expressions that are uniform in all languages would facilitate very much international understanding and convenience.

An article published by a noted mechanical engineer of New York attempts a similar form of argument by imagining a child sent to the grocery to purchase a kilogram and 850 grams of cheese, and questioning the effect of such a demand upon the parents and the grocer. That was simply trifling with the subject. If we stop for a moment and reflect that a gram is a very fine subdivision of weight, more strictly comparable to the grain than to the ounce, wouldn't it be equally absurd for a child so instructed to ask the grocer for a pound, 10 ounces, 6 drachms and 20 grains of cheese; or if in search of some commodity the druggist dealt in, to ask the latter for a pound, 6 ounces, 5 drachms, 2 scruples and 6 grains of his commodity? Would not the merchant be at least as much astonished by the latter requests as the former in the metric measures? Arguments of that kind are very funny, but not logical. If the metric system were in vogue for the ordinary purposes of life, we would do just as the French and Germans do. We would speak of the halves and quarters of the meter or the kilogram or the liter, just as we do with our decimal coinage, without any inconsistency.

The gentleman who preceded me criticised the millimeter as being

too small a unit for popular convenience on hand rules. This doubtless depends upon the point of view. To many others the millimeter appears to be a very convenient minimum unit. Our ordinary rules are usually divided into sixteenths of an inch, which is too large a minimum unit, and the half of this, or the thirty-second, is rather too fine a subdivision for ordinary rules. From a broader point of view we might state that if the matter of the units of subdivisions on rules, or facility of measurements, or convenience of workshops, were the only questions involved, we do not need to make any change, our present system is good enough. But we must look further; we must consider the army of men who make our computations, burdened as they are with our incongruous systems. Also the convenience and economy of those who are engaged in the world's commerce. We desire to facilitate international trade relations and remove all the barriers that we can with consistency, to permit international exchanges. If an international system is to be sought we surely cannot expect those who have discarded their ancient systems and adopted the metric standard to drop this and take up our heterogeneous methods. By reading the report of the committee you will observe that no compulsory measures are proposed or suggested. It is not probable that any system could be forced upon the community, against the popular desire, by any legislative enactment, and yet the very nature of the case implies that to make it effective it must be adopted by all; there can be no half-way measures in connection with any system of units, in which the members of the community have to deal with each other, any more than we could have several languages co-existent as a national tongue. It is quite possible, however, for the National Government, which is the largest consumer of the country, to put it in force in its own Departments; it exists already in some of these Departments. All that is necessary is to enforce it in others as rapidly as possible, without serious expense or confusion to the interests involved. If the National Government would do this, the great army of manufacturers who are supplying the national wants would necessarily adopt the system in their relations with the Government. This would surely lead to its adoption in our relations with each other. The movement already has a strong following in Great Britain, and it is not improbable that the British may precede us in its adoption, and when the system comes into popular use, the only wonder will be that we so long delayed in accepting so convenient and time-saving a system. (Applause.)

MR. SPENCER FULLERTON:—In the case of the adoption of these measures, would the sizes of articles in general use be changed? I mean by that, for instance, in your business, Mr. Christie—you make I-beams and channel irons—would the dimensions of those products be changed to the nearest equivalent in the metric system? For instance,

a 4-inch channel might actually be altered to measure $3\frac{9}{10}$, or whatever the fraction was, to agree with the decimal and points of the metric measure; and would the whole range of sizes of our rod and bar irons be altered?

MR. CHRISTIE:—I do not see any reason why there should be any change at all. I think we should simply take them as they are and give them the nearest metrical measurement. I think in time we would adopt new sections. That would come in time; but it would cost something to do it. We are making new sections and new tools all the time.

Why should we change our screw-threads because of adopting the metric system? There is not a nation in the world but what uses the United States standard screw-thread. A system of metric screw-threads has been proposed and may be adopted; but the reason it has not been adopted is because there is no necessity for it. We can govern that and maintain our own, and keep to the present standard. We do not need to change the present standard of screw-threads.

MR. JESSE PAWLING, JR.:—I beg leave to make a statement. I was present with Mr. Bond at the meeting of the Committee on Coinage, Weights and Measures, and I can affirm positively, from remarks made by Mr. Southard* and Mr. Shafroth† after the hearing, and also from conversation with Dr. Stratton, the Director of the National Bureau of Standards, that it is not the intention of the framers of the Bill to force the metric system upon the country; it is not their intention to make this system compulsory; and, whether this bill is passed or not, the English system can be used in any machine shop as long as the owners see fit. And I beg leave to state another point: At that meeting of the Committee on Coinage, Weights and Measures a strong effort was made to obtain from those who were interested in machine tools an answer to the question: where would the expense be incurred in the change? After considerable questioning, it was brought out that such tools as taps, dies, reamers, etc., would need to be changed. This question was asked by Dr. Stratton, who is familiar with machine tools, as well as being a scientific man: If the metric system were made compulsory, what change would be required to be made in a shaper, for example? It was answered that no change need be made in floor tools.

THE CHAIRMAN:—The Institute would be glad if Dr. Houston would participate in the discussion.

DR. EDWIN J. HOUSTON:—Mr. President and Gentlemen, I came rather to listen than to talk. This proposition appeals to me as a scientific man, and from that viewpoint there can be no question as to how I would vote if I had to vote; that would be for the adoption of the

* Hon. J. H. Southard, Chairman of the Committee on Coinage, Weights and Measures, of the House of Representatives.

† Hon. J. F. Shafroth, who introduced the bill, H. R., 123.

metric system. We have passed the time, I think, in this country when we can gravely continue to announce to the world that our unit of length is based on the length or the space occupied by three barleycorns placed side by side. There might have been a time in the world's history when three average barleycorns grown in a limited region could be got that together would measure an inch with approximate exactness, or thirty-six of those barleycorns would equal 1 foot; but that time has long since passed. Of course, as a scientific man, I cannot but feel that we ought to adopt the metric system. As an electrician, with the practical units of electricity being metric units, there can be no doubt as to the advantage that would accrue by the adoption of the metric system. When we change our lineal dimensions to areas and volumes, or to squares and cubes, then the disadvantages of our system become more and more apparent, and I should hope to see some movement taken by the Institute to urge upon Congress the adoption of the metric system.

I can readily understand, however, the position which Mr. Bond, the representative of the Pratt & Whitney Company, took that there would be a difficulty in carrying out this change if it were made too suddenly, and I can even appreciate the difficulty that he and other men might experience in endeavoring to carry out practically those large decimals of small units of length. Still, the advantages of the metric system are so great that I do not think that any one or two classes in the community should expect the great body of the community—the working scientific men, the practical scientific men of the world and the public at large, let alone the coming generations—to continue indefinitely in the use of the old system. The chairman of the committee has pathetically alluded to the additional difficulty to the work of the schoolboys—he even included the girls—and I quite agree with him that we ought to have a less barbarous system of units of length, weight and measure generally; therefore, I am heartily in favor of the proposed change; but I think that any change that is to be made should not be made instantly obligatory on the entire community, but that a reasonable time should be allowed for its adoption, so that great shops could gradually change their patterns, pitch of screws, standards, etc. On the whole, I am decidedly in favor of the change recommended by the committee.

THE CHAIRMAN:—The Chair was not without guile in selecting this committee, because he felt that, just as Dr. Houston says, the technical men would favor the metrical system. On the other hand, he had reason to believe that if there was a weak point in that system, it would be found out by the practical men who have learned their trade at the bench and have handled the tools and who had to deal with them; and if you noted the composition of the committee, you will observe that it is very evenly divided between the two classes. Mr. Christie happens

to represent a company that has not only built bridges for this country, but we have all been glad and proud of the fact that they have gone clear over into Egypt and done notable work there. We might go even further, because the Baldwin Locomotive Works have sent their products all over the world, and I think the members will agree with me that Mr. Vauclain, superintendent of those works, is perfectly competent to speak of the merits and demerits of the metrical system.

MR. S. M. VAUCLAIN:—I was unfortunate enough to be selected as a member of this committee. The committee found, when they assembled together, that they were practically all of one mind, although they took several ways of expressing themselves. You will notice in the appendix to the report of the committee—No. 9—we say, if in the course of a term of years the system came into universal use in the service of the Government, it is probable that its adoption would follow elsewhere within a reasonable time. This paragraph made it possible for all the members of the committee to unite in signing the report, and the fourth paragraph of the report, the second resolution, "That the National Government should enact such laws as will ensure the adoption of the metric system of weights and measures as the sole standard in its various departments as rapidly as may be consistent with the public service," will make it apparent to you that this committee was a unit against any compulsory measure being enacted by Congress to enforce the metric system upon the manufacturers of this country.

So far as the metric system is concerned from a manufacturer's standpoint, it certainly should have no terrors. Where—in what workshop—can you find a dozen men who will measure the same piece of work and find the same result with the ordinary 2-foot rule, or such scales as are ordinarily provided for their use? Could any manufacturer in America to-day rely upon the accuracy of the measurement of its employés in its product? Instead of having first-class fits and interchangeability he would have first-class misfits and ruination of his trade.

We have a moderately large workshop, employing some 11,500 men; our work is very much in detail; the first-class locomotive of to-day requires 13,000 separate pieces, all accurately made, before that locomotive can be turned out on the track as a finished product; and if we stop to consider that five finished locomotives of this kind are turned out every working day in the year, it can readily be understood how poorly these locomotives would be fitted together if we relied upon each and every one of these 11,500 men to do the measuring necessary to fit these parts together with the drawings furnished by the draftsmen in their hands.

What is the natural procedure, then, in a workshop of this kind? You receive the drawings from the drawing-room; they are all made to, we will say, the English measures—12 inches to the foot, 3 feet to the

yard, or whatever you please—no matter how you may see fit to speak of it; but really and truly these drawings are not made to the ordinary English measure; they are made to a scale which is adopted, and which represents 12 inches to the foot, or 3 feet to the yard, or so many sixteenths inches to an inch. The scale that we have adopted in our drafting-room is a scale of 2 inches to the foot, and in comparing everything that we look at, we do not consider the foot at all; but if it is 2 inches long it is a foot long.

Therefore, is it not apparent to any one here that if these designs are made according to the metric system, it would be just as easy for us to judge of the size of an article by the dimension on the metric drawing as it would be upon the drawing that is made to conform to our present system of measurement? Granting that that is the case, and the drawing is placed in the shop, the holes are marked the nominal diameter; we will have an inch hole marked upon the drawing, one-inch-and-an-eighth hole, or an inch-and-a-quarter hole. Now, do we put inch-and-a-quarter holes or inch-and-an-eighth holes in this work? Not at all; the holes are almost any size. These sizes are governed by a set of standards which is adopted by the workshop for the bolts that are to fit in those holes; and in order to get a tight-fitting bolt we are not foolish enough to undertake to fit a straight bolt in a straight hole where several straight holes come one above another in several pieces and expect to get a good fit all the way through; we adopt a taper bolt the nominal diameter of which is one inch, or one and an eighth, or one and a quarter; and when the hole is drilled it is drilled the nearest size to the smallest part of the reamer which is to pass through that particular piece; and then the pieces are reamed to a set of standard gauges and never measured at all. I doubt, if you would go through a workshop containing three or four hundred men, whether you could find a dozen first-class two-foot rules in the establishment, and a steel scale has grown to be a curiosity. Of course, a steel scale is necessary; but it should be in the standardizing department. Where these scales are compared and seen to be of the proper size, and where all the calipering-gauges for turning work to proper diameters are made and corrected, the use of a measuring stick (and a two-foot rule is nothing more than an ordinary measuring stick) is merely to approximate the diameter. The pair of calipers will be set probably within a sixteenth of an inch for the rough cut; but the finishing cut is always calipered or measured with a positive gauge that is furnished by the measuring department or those who we suppose know how to measure.

Some few years ago I was not inclined to favor the adoption of the metric system, and I cannot say to-day that I am in favor of it. We can get along with the ordinary foot. It doesn't make any difference, particularly, how long a foot is, so that everybody uses a foot that long.

We could just as well get along with the meter. It makes no difference whether the meter has a positive length; that can be determined accurately by scientific measurement. The meter might just as well be 40 inches long, or it might be 36 inches long; it don't make any difference so that that meter will be divided by a decimal system. We think that it would probably be much more convenient than the system we have now of dividing up the foot, which we now use; and, feeling that way about it and the adaptability of any system to the workshop, I really have no objection to the adoption of the meter.

It resolves itself into this: that in the scientific world of to-day—a world that we did not enjoy thirty or forty years ago—those of us who have lived more than fifty years will remember when we were boys and going to school we did not have and we were not compelled to learn those things which the average boy is compelled to learn now before he can start out into the world to undertake to earn a living; we had very little electricity—very little, indeed; we were just commencing to know something about it; and when you come to talk about telephones, about the economical consumption of steam, and a dozen other very important matters that are absorbing the public to-day, you will wonder what we did have to study when we were boys.

Furthermore, it seems to me that we can look upon this matter somewhat in this light: if we were to go to Germany—any one of us—to start a workshop, we would not undertake to start it on an English-speaking basis; we would learn the German language, and we would do our business in German. Now, if we are going to adopt the metric system, all we will have to do will be to learn to think in the metric system. You can go in this city to-day to a school of languages and in a few weeks learn to speak almost any foreign language sufficiently well to enable you to get along. I have known men who have been able to master enough of the German language in three weeks to get along splendidly in Germany. How is this done? This is done simply by the professor or teacher compelling the party or person who desires to acquire a knowledge of the language, to stop thinking in his own language and to think in the language that he is going to adopt, or temporarily adopt. When he is compelled to think in that language, it is a very easy matter to teach him to speak in that language; and therefore, instead of making our articles 4 or 3 feet long, we would make them a meter long, or a meter and a half long; everything that we would make we would think about in accordance with metric sizes and metric dimensions.

The gentleman from the Pratt & Whitney Company did not enlighten us fully or sufficiently well to give us an opportunity to give proper weight to his remarks. He failed to tell us how expensive the change would be. He said that it would be expensive; he said that it would

be troublesome, but did not tell us how expensive or how troublesome. When a change of this kind would commence in any manufacturing establishment, it would first commence in the drawing-room (because unless the drawings were made in accordance with the metric system, the men in the shop could never work to it), and there would be very few gauges in use in the shop that would ever have to be changed, because the gauges do not depend upon the figured dimensions on the drawings; the drawings would all be figured for the gauges. A certain gauge would be called for instead of a certain dimension. In our works to-day there is not a single hole drilled in a connecting rod where the straps are fitted on the stub ends of the rods, that is drilled to a dimension; the drawings do not refer to any dimensions; we have no use for dimensions, but we have for gauges. They are marked to be drilled with a certain gauge and a certain bushing piece. You could not use an inch and a quarter drill in an inch and an eighth bushing. Whatever bushing you use determines the size of drill that you are going to use; and whatever gauge you use determines the distance apart the holes may be and the number of them, and the distance they are from the end to the stub. The workman goes ahead and drills regardless of consequences, in accordance with the gauge that is ordered on the drawing; and the result is that these parts are perfectly interchangeable, and hundreds and thousands of these parts are duplicated from time to time and shipped to almost every country on the face of the earth, and that without a single dimension, either metric or English, on the card—simply the gauge number calling for that part. This may be met with the remark that those people who do not do their work with gauges would not find it so easy to change; but that is easily confronted by stating that no first-class shop, or any shop, no matter how small it might be, that desired to enter into competition with the world, would ever do its work in any other way and expect to succeed; it would die a natural death sooner from the mere fact that it failed to use gauges or jigs for the output of its work—even though it had only one of a kind to make—much sooner than it would if it undertook to use the metric system. (Applause.)

THE CHAIRMAN:—The Chair has a list of gentlemen here whom he would be glad to call on, but he is reminded that the hour is getting late. You have the resolution before you, which is entirely in your hands as to what disposition you shall make of it. The matter can be disposed of to-night; this discussion can be continued; it is for the Institute to decide.

A. E. KENNELLY:—I move that the meeting adopt the report and take such steps in connection therewith as the resolution and report call for. (Motion seconded.)

THE CHAIRMAN :—The motion is that the meeting adopt the report of this committee and take such steps as the resolution and report call for. The question has been called for: are you ready for the question? Those in favor of the motion will say aye, those opposed, no. The ayes appear to have it. The Chair decides that the ayes have it. (Applause.)

This resolution will now go forth with the endorsement of the Franklin Institute, and the Secretary will see that the voice of the Institute as expressed to-night is properly presented.

THE SECRETARY :—I should like to have the views of the members regarding the publication of this mass of correspondence, some of which is exceedingly interesting—both that in favor, that against, and that non-committal. I assume that it should be edited in some way—possibly all of it—for publication.

DR. HOUSTON :—I move that it be referred to the Committee on Publications, with power to act. (Seconded.)

THE CHAIRMAN :—The motion is before you that these contributions be referred to the Committee on Publications, with power to act. (Carried.)

[*To be concluded.*]

## FUEL OIL ON SOUTHERN PACIFIC RAILROAD.

It is reported that the Southern Pacific Railroad is making elaborate preparations for the use of oil as fuel through that system. The company intends to establish seventy-two steel tanks of 50,000 barrels average capacity along its lines, and has let the contract for this work. These tanks will have a capacity of 3,600,000 barrels, and these, together with the thirteen already constructed by the company, will give a total capacity of 4,425,000 barrels. Julius Kruttschnitt, first assistant to the President of the Southern Pacific Company, is authority for the statement that the company intends to use oil for generating power on the locomotives from one end of the line to the other, and eventually to substitute oil for coal fuel on engines, ferryboats and steamboats. Already the company has converted 210 locomotives into oil burners and has on hand material at the Houston shops for converting 120 more.—*Iron Age.*

## PHYSICAL AND CHEMICAL PROPERTIES OF GELATIN.

At the meeting of the Academy of Science of St. Louis, on the evening of January 20th, Dr. George Richter delivered an address on the "Physical and Chemical Properties of Gelatin," which he described as a spongy substance differing materially from other solids. The manner of manufacture of gelatin and its chemical and physical characters was described in detail, and considerable attention was given to the rate of absorption and evaporation of water by gelatin, and the phenomenon of its apparent solution in water. A new hygrometer was exhibited and described, the action of which was based upon the water absorption of gelatin. W.

# THE FRANKLIN INSTITUTE.

*A lecture delivered at Association Hall, February 8, 1902.*

## THE GASES OF THE ATMOSPHERE.*

By Dr. H. F. Keller,
Member of the Institute.

That distinguished chemist, who has added more than any other living man to our knowledge of the earth's atmosphere—Prof. William Ramsay, of London—has said that to write the history of the development of this subject would be to write a history of chemistry and physics.

While this remark must, of course, not be taken literally, it is certainly true that the early investigations on the nature of air coincide largely with the beginnings of physical science, and also that the progress in the study of the atmosphere has steadily kept pace with the advance of the two great branches of this science. No other subject has received greater attention at the hands of physicists and chemists during the past three centuries, and yet this field of research is far from being exhausted, for it continues to yield rich harvests to those who are tilling it in our own days.

It is on account of the remarkable discoveries that have been recently made, that I have chosen the subject for this lecture, in which I purpose to tell in popular language the story of the rise and development of men's ideas concerning the composition of air, and to illustrate this story with a number of experiments and lantern slides.

Much of what I shall say will doubtless have a familiar sound to a large part of this audience, and I must ask their indulgence to enable me to give a connected account for the benefit of those who are not acquainted with the evolution of atmospheric chemistry.

* In the preparation of this lecture free use has been made of the standard works on the history of chemistry, and especially the writings of Professor Ramsay on this subject.

According to a view which originated with the famous philosopher, Aristotle, the preceptor of Alexander, all material things in the universe were made up of four elementary principles or elements, namely, earth, air, fire and water. Each of these elements was supposed to be endowed with and characterized by, certain fundamental qualities which it imparted to those bodies into the composition of which it entered. Thus earth was cold and dry; air warm and moist; fire, on the other hand, warm and dry; and water cold and wet.

This doctrine was universally accepted as a fundamental principle in philosophy, and survived, in variously modified forms, until the middle of the seventeenth century, and some remnants of it can indeed be traced to a much later period.

As an illustration of how it was applied to explain natural phenomena, let me quote a passage from a "Manual of Astronomy," written in Anglo-Saxon a thousand years ago: "There is no corporeal thing which has not in it the four elements, that is, air and fire, earth and water. . . . Take a stick and rub it on something, it becomes hot directly with the fire that lurks in it; burn one end, then goeth the moisture out at the other end with the smoke." And I may add that the ascending smoke, and the ashes remaining after the wood is burned up, were regarded as evidence of the presence of air and earth in the stick.

Strange as the doctrine of the four elements may seem to us at the present day, it endured longer than any other philosophical teaching; its overthrow we owe to the great experimental philosopher, Robert Boyle, who, in his book, "The Sceptical Chymist," published in 1661, advanced convincing arguments to prove that Aristotle's elements (and those which had been added by the alchemists) cannot be regarded as "certain primitive and simple bodies which, not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved"—a definition which agrees perfectly with our modern conception of the elements as substances

out of which two or more different substances have not been obtained.

It is to Boyle also that we are indebted for one of the most remarkable early attempts to ascertain the nature of the atmosphere. In his "Memoirs for a General History of Air," he sets forth his reasons for considering air to be a mixture of "at least three kinds of corpuscles," attempts to explain its action upon burning substances, and proposes numerous experiments by which our knowledge of air might be advanced.

But while he made many most important contributions to the *physical* side of the subject, and clearly recognized air as a material substance, he did not succeed in discovering its composition, or in ascertaining its relation to combustion or respiration.

The first steps along these lines were made by his contemporary, John Mayow, an English physician. In a now famous treatise, which he wrote in Latin and published in 1674, this acute thinker endeavors to prove that air consists of two kinds of particles. By close observation and shrewd reasoning he is led to believe that only one of these constituents, the *nitro-aerial particles*, is necessary for supporting life and the burning of inflammable substances, and that the other, which remains after this active constituent is removed, is incapable of supporting either respiration or combustion. He asserts that the "fire air" enters the blood during respiration, and is the mainspring of motion in animals and plants. By well-conceived experiments he shows that saltpetre and various acids contain the "nitro-aerial particles;" that it is this constituent which is absorbed when a burning candle is placed in air confined over water, and that the residue left after the fire air has been consumed is lighter than fire air. He argues that a combustible cannot burn in a vacuum, that is, in the absence of air, unless it contains its own supply of fire air, as gunpowder, for example.

Mayow communicated the results of his studies to the Royal Society, but they received scarcely any recognition from his contemporaries, and were quite forgotten when, a

century later, a greater mind, equipped with a fuller knowledge, compelled the scientific world to accept views which are almost identical with those advocated by Mayow.

At the time of Mayow's death, which occurred in 1679, the only facts concerning the atmosphere that had been definitely established, were:

(1) That it consists of matter, and, therefore, possesses weight and exerts pressure upon the surface of the earth.

(2) That it is a fluid which, however, differs from other fluids, such as water, in that it has "a spring or elastical power," that is, will yield to external pressures, but regain its original volume when the pressure is removed.

(3) That it is not an element, but composed of at least two kinds of particles; and

(4) That it is in some way connected with the processes of combustion and respiration.

More than half a century elapsed before any further progress was recorded. In 1727 an English clergyman, named Stephen Hales, published some essays on "Vegetable Staticks," in which he gives "a specimen of an attempt to analyze air by a great variety of chymiostatical experiments," and describes at great length his observations of the influence of air on the growth and development of plants. From our point of view this book is of interest, as it shows that Hales was a skilful experimenter, who devised new methods and apparatus to produce and study various gases. We still follow his example in employing separate vessels for generating and collecting gases, and there is no doubt that Hales prepared, in a more or less pure condition, a considerable number of different gases, but he failed to perceive wherein these gases differed from common air. His prejudiced mind was satisfied to regard them as modified atmospheric air, and he concluded that the latter is a "chaos, consisting not only of elastick, but also of unelastick particles, which in plenty float in it."

We miss in Hales' researches the guiding hand of theory, and nowhere, perhaps, in the history of science is the value of theory better illustrated than by the subsequent developments in our subject.

About the time when Hales' book appeared, a very remarkable though erroneous doctrine, intended to explain the nature of combustion and allied phenomena, had come to be generally accepted. The *theory of phlogiston*, as it is called, originated with the German philosopher, John Joachim Becher, but was further developed and definitely formulated by another German, George Ernest Stahl. In this doctrine it was assumed that all combustibles are compound bodies containing a certain subtle principle, the phlogiston, which makes its escape when the substance burns, while the other constituent is left behind either in the form of an acid or as an earthy powder or calx.

*Experiment.*—Thus, when sulphur or phosphorus is burnt, the phlogiston was supposed to leave these substances and pass into the air, sulphuric or phosphoric acid remaining; and metals, such as tin or lead, when ignited, would yield up their phlogiston and be converted into calces, a transformation which to this day is spoken of as *calcination*.

Non-combustible substances—lime, for instance—on the other hand, were supposed to be calces; and it was believed that if the phlogiston were restored to them, they would again become combustible. As to what this mysterious principle really was, the ideas of chemists seem to have been very vague, and to have changed from time to time.

A sound theory must include all the known facts concerning what it professes to explain, but the theory of phlogiston had a weak spot in its failure to account for the gain in weight of metals when they are calcined, a fact which had long been demonstrated.

The improved facilities in experimentation created by Hales, and the phlogistic theory, combined to stimulate chemists to renewed activity in the study of gases and the atmosphere. During the second half of the eighteenth century the leading investigators directed their energies mainly along this line. Important discoveries followed one another in rapid succession: different "kinds of air," or gases, we would say, were obtained and carefully examined; atmospheric air was recognized as a mixture of such gases, and the relative amounts of these were accurately determined;

and, finally, the chemical changes in which the atmosphere takes part—the phenomena of combustion, calcination and respiration—were correctly explained.

This period in chemistry has, therefore, properly been called the *pneumatic period.* The great discoveries which were then made form the foundation upon which has been reared the great edifice of chemical science.

I shall not attempt here more than a very brief sketch of the great achievements made during this period, for I desire to dwell more particularly upon some of the recent discoveries.

Five great names stand out prominently among those of the chemists of the pneumatic period. They are Joseph Black, Joseph Priestley, Carl Wilhelm Scheele, Henry Cavendish and Antoine Laurent Lavoisier. Of these the four first named may be said to have contributed the principal discoveries of facts, while to Lavoisier, who was scarcely inferior to them as an experimentalist, belongs the glory of having given the true interpretation of their results.

Before the middle of the eighteenth century no aeriform fluid or gas had been clearly distinguished from common air. The gases which Hales and others had obtained were regarded as *modified* air, differing from pure air much in the same way as natural water differs from distilled.

The discovery by Black, in 1751, of "fixed air," or carbon dioxide, was the first instance of a gaseous substance to be recognized as different from air, and, at the same time, as a constituent of the latter. Black obtained it from magnesium carbonate by means of Hales' apparatus; he heated the substance in a bent gun barrel, and collected the gas given off, as I am doing now, over water. In 1755 he published his celebrated thesis entitled, "Experiments on Magnesia Alba, Quicklime, and other Alcaline Substances," a model of inductive reasoning and experimental inquiry, in which he establishes the nature of carbon dioxide as a distinct substance, and proves that small amounts of it exist in air.

*Experiments.*—A few experiments will help us see the difference between "fixed air" and ordinary air.

(1) A burning taper or candle is extinguished at once when brought into carbon dioxide.

(2) This gas is much heavier than air, for you see that a soap-bubble will float upon it.

(3) And we can always recognize its presence by this simple test, that it turns clear lime water turbid or milky.

In 1766 an important paper was published by Cavendish under the title of "Factitious Airs." It contains, among other things, the first account of "inflammable air," afterwards called hydrogen. This gas was made by the action of acids upon metals, and its discoverer, being like most of his contemporaries, an adherent of the phlogistic theory, expressed the opinion that it either was or contained phlogiston, and that its source was in the metals. While this turned out to be erroneous, Cavendish describes very accurately the principal properties of the new gas, and characterizes it as an individual substance. Many years later it was recognized as an element, and quite recently the French chemist, Gautier, has proved that the atmosphere contains an exceedingly small amount of it.

*Experiments.*—(1) To illustrate the properties of hydrogen, I prepare it by pouring sulphuric acid on pieces of zinc contained in this generator.

(2) The gas being very much lighter than air, a soap-bubble filled with it will quickly rise to the ceiling; and

(3) We can easily identify this gas by igniting it; it burns with a non-luminous flame.

In 1772 Cavendish obtained another important gas—"mephitic air" or nitrogen; but the same discovery was simultaneously made by a pupil of Black—Daniel Rutherford—whose dissertation on the subject appeared in the same year, and Cavendish, quite indifferent as to the credit due him, made no claim for priority.

Both chemists made this gas in the same way: they passed ordinary air over glowing charcoal, and then removed the carbon dioxide so produced by absorbing it with lime or potash. They noticed that the remaining gas extinguished flames, was not combustible, and had no effect upon lime water.

*Experiments.*—(1) I have here some jars filled with nitrogen; you see how the flame of this candle is smothered in the gas, and that the latter is not inflammable.

(2) Clear lime water poured into the other jar does not become cloudy.

Rutherford considered nitrogen as a kind of phlogisticated air, although it is incapable of burning.

The discovery of the most important constituent of air, that of oxygen, followed next. It was made independently by Scheele in Sweden, and by Priestley in England. It furnished the clue to the true nature of the atmosphere, and led to a complete revolution in the views of chemists. Without exaggeration it may be said that it marks the beginning of scientific chemistry.

It is interesting to compare the ways by which Priestley and Scheele arrived at the same result. The former was a clergyman, who made experiments as a relaxation rather than for any serious purpose, and while he did make a great number of brilliant discoveries, he lacked in thoroughness and scientific ability to draw the proper conclusions from what he observed. Scheele, on the other hand, was a born investigator, who conducted his experiments with definite ends in view, and whose acute intellect was able to deduce the true meaning from their results.

These differences are clearly reflected in the writings of the two chemists.

Here is Priestley's account of his discovery of oxygen: "Having procured a lens, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of substances would yield, putting them into vessels filled with quicksilver, and kept inverted in a basin of the same. I endeavored to extract air from *mercurius calcinatus per se;* I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found it was not imbibed by it. But what surprised me more than I can well express, was that a candle burned in this air with a remarkably vigorous flame. I was utterly at a loss how to account for it."

In contrast with this hap-hazard manner of experimenting, Scheele is led to the discovery of oxygen in the course of a research in which he seeks to establish a theory of the nature of fire and to find an explanation of the part played by air in the phenomena of combustion. In his famous "Treatise on Air and Fire" he shows that air consists of at least two kinds of elastic fluids, one of which, the *vitiated air* or nitrogen, remains when the other, which he calls *fire air*, has been removed by combustion, respiration, or chemicals. He describes how this fire air or oxygen can be obtained by heating various substances, such as nitre and oxide of mercury, and then goes on "to prove that ordinary air, consisting of two kinds of elastic fluids, can be compounded again, after these have been separated from one another." He shows conclusively that oxygen made from calces is identical with that in air, and determines the principal properties of the gas.

He also estimates, though only roughly, the relative amounts of oxygen and nitrogen contained in air, and describes many interesting observations on the respiration of animals.

*Experiments.*—(1) We can readily obtain some oxygen by heating chlorate of potash in a glass tube.

(2) The distinguishing characteristic of this gas is that it supports combustion better than ordinary air; you notice that this glowing chip bursts into flame in it.

(3) A burning candle emits more brilliant light; and

(4) A piece of phosphorus is consumed with dazzling splendor.

I have pointed out before that the great activity in chemical research during the pneumatic period must be largely credited to the phlogistic theory. Its sway continued unbroken while Scheele and his English contemporaries made the great achievements of which I have told you.

Not only did this (to us) absurd hypothesis completely dominate the mode of thinking, it determined the very form of expression. It would be quite impossible for us to read intelligently the chemical writings of those times without

having a knowledge of its conceptions and terms, just as no one can hope to make much headway in the chemical literature of to-day without being conversant with the atomic theory, which is the basis of our chemical philosophy.

It is to the genius of Lavoisier that we owe the overthrow of the reign of phlogiston, and its replacement by the sound reasoning which has lifted chemistry to the rank of a true science.

Even prior to the discovery of oxygen, and before the exact composition of air and water had been ascertained, this master mind was able to renounce the phlogistic doctrine, and to declare it at variance with the fact that the substances produced when bodies burn weigh *more* than the combustibles themselves. The discovery of oxygen (of which he learned in 1774, the year when Priestley made it), and experiments of his own on the calcination of metals, enabled him to formulate a new theory of combustion, according to which only one of the constituents of air—oxygen—is capable of absorption by the burning body, while the other—nitrogen—is incapable of supporting combustion. By argument as well as experiment Lavoisier met his opponents, and finally succeeded in converting most of them to his views.

The *New Chemistry*, as Lavoisier's system came to be called, undertook to explain, and with complete success, all the facts of a chemical nature known in those days, and its essential features have stood the test of experience to the present day.

In his "Elementary Treatise on Chemistry," published in 1789, Lavoisier produced a book which, in many respects, has served as a model to all who have since written upon the subject; his views are made the basis of a new arrangement of the facts, which he states with marvelous clearness in a new language from which the terms of the discarded doctrines are rigidly excluded.

Lavoisier also showed that respiration is analogous to combustion, that it is, in fact, a process of slow combustion in which oxygen is consumed, and carbonic acid exhaled with the unconsumed portion of the oxygen and the nitrogen.

I have yet to speak of one of the most important researches on the composition of the air which was in progress while Lavoisier's ideas were taking shape, and which largely contributed to make these ideas more precise. I refer to the classical "Experiments on Air," which Cavendish began in 1777, and published in the years 1784 and 1785. In these researches by one of the greatest experimenters the world has ever known, the atmosphere was proved to have a constant composition, the relative amounts in it of oxygen and nitrogen being determined with marvelous exactness. The process employed by Cavendish for this purpose had been previously used by other chemists, but with results very different from those which he obtained. For, while Priestley and the others had come to the conclusion that air collected in various kinds of weather, and in different localities, varies in its composition, *he* found as the result of a large number of analyses that, after removing the small amounts of carbonic acid it contains, air is made up of

| | |
|---|---|
| Nitrogen (phlogisticated air) | 79·16 per cent. |
| Oxygen (de-phlogisticated air) | 20·84 " |

results which differ but very slightly from those of the best modern determinations.

In the course of these investigations Cavendish made other discoveries of far-reaching consequence; he showed that water is not an element but a compound, consisting of hydrogen and oxygen, established its exact quantitative composition and also that of nitric acid, and, as we shall presently see, obtained argon in an impure state, without, however, recognizing it as a peculiar gas.

With the labors of Cavendish and Lavoisier the investigation of the atmosphere was, for some years at least, regarded as practically completed. The mysterious element of Aristotle, and "chaos" of Hales, had been recognized as a mixture in definite proportions of oxygen and nitrogen gases, containing also very small amounts of watery vapor, carbonic acid and ammonia. The chemical changes that take place when combustible substances burn in air, when metals are calcined in it, and when animals breathe, were

clearly understood; it is the active constituent of air, the oxygen, which combines with the combustible or the animal matter, while the nitrogen simply restrains this activity by diluting the oxygen.

The nature of air was thus established in its main features, but an enormous amount of work was to be done in the following century to make our knowledge of the atmosphere more exact and complete.

Before proceeding with my story, I wish to exhibit on the screen a number of pictures to illustrate the part I have told.*

Returning now to where we left the thread of our narrative, we enter once more a period of great activity in the experimentation with gases. In 1804 Gay Lussac and Humboldt made a large number of very careful analyses of specimens of air collected in many parts of the world, at different altitudes and in all kinds of weather. They employed a new process of analysis which they believed was more accurate than that of Cavendish, but the results were nearly identical with the English chemist's; the ratio of oxygen and nitrogen was always the same, 100 volumes of air being found to consist of 21 of oxygen and 79 of nitrogen.

Constancy of composition having been shown to be essential to chemical compounds, was it not likely that the constituents of air were chemically combined rather than simply mixed?

Further experiments by Gay Lussac revealed the fact that whenever two gases combine to form a compound, they do so in a simple proportion, such as 1 volume of one gas to 1 volume of the other, or 1:2, or say 1:5; but no such simple proportion existed between the volumes of the oxygen and nitrogen in air.

[*To be concluded.*]

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* The slides shown included portraits of nearly all the chemists who had been named, as well as pictures illustrating their lives and labors.

# Mechanical and Engineering Section.

*Stated meeting held Thursday, October 16, 1901.*

MR. JAMES CHRISTIE in the chair.

## THE CONSTRUCTION AND INSPECTION OF STEAM BOILERS; WITH ESPECIAL REFERENCE TO THE "CITY OF TRENTON" DISASTER.

(*Concluded from p. 340.*)

### DISCUSSION.

MR. WASHINGTON JONES:—How would you, Mr. Vauclain, determine that that boiler had no water in it, and whether the water was below the crown-sheet?

MR. VAUCLAIN:—It would be a very difficult matter to determine just how much water was in this boiler, or as to how far it was below the crown-sheet. The Government boiler inspector testified that he thought the water was 10 inches below the crown-sheet; but it is my honest opinion that the Government boiler inspector did not know anything about it; and if you would like to have my opinion, my opinion is that the water, from what I have seen—I have no evidence—even though one man testified under oath that the oiler told him that they had low water, and the oiler testified under oath that that was not true—my idea is, gentlemen, that the water had either reached, or not quite reached, the top of the crown-sheet, and that the intensity of the flame in this section treated this very little water—and the water acted on the sheet—just as water would on a hot stove-plate; consequently, this portion of the sheet got hot (*Fig. 17*), and if there had been that small amount of water it would have helped to keep those portions cool which were protected, as this would be where the flames would pass around. Any one who has noticed the flames in an ordinary heating furnace in a blacksmith shop would know just exactly how that flame would travel, and that the after-portion—where the fire very little reached—

the water would bank up on there; consequently this water would be driven out and driven away. Will it cause the trouble? It would, because a very slight heating in that plate would interfere very seriously with the holding power of one of those studs; not so much on account of the number of studs intact, but on account of the sagging of the plate—the slight sagging of the plate—which would loosen the tightness of the fit, if it was tight on those studs; and when we criticise the production of any well-regulated manufacturing establishment, it is only fair and just for us,

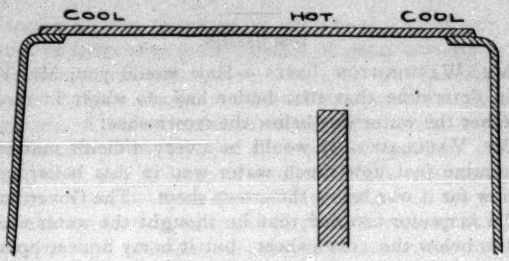


FIG. 17.

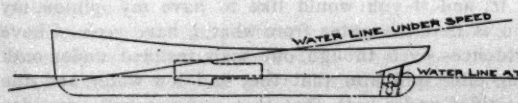


FIG. 18.

without other evidence at hand, to feel that the work was done properly.

Also, inasmuch as this work has been tested by inspectors appointed by law and, as I understand, was supervised by others who were watching and who were interested in the manufacture of this particular boiler and the boat itself; therefore, if the boiler had passed through the scrutiny of these various persons who were entrusted with the character of the work that was being done on this boiler, and tested and passed and fired and put in service, certainly the boat was not turned over with any knowledge on the part of any

one—either employed by the Government or by the manufacturer who made it—that there was anything in this boiler that would interfere with it giving good service; and it is my honest opinion—whilst I have not built a boiler just like this (we do not build boilers just like this for locomotive work), I would not be afraid to use a boiler constructed like that in the stationary service or ordinary service where I would always be able to have sufficient water over the crown-sheet.

Mr. Chairman, there is another matter in connection with this that has been lost sight of. I am not a shipbuilder; I do not know whether I can draw a ship (*Fig. 18*).

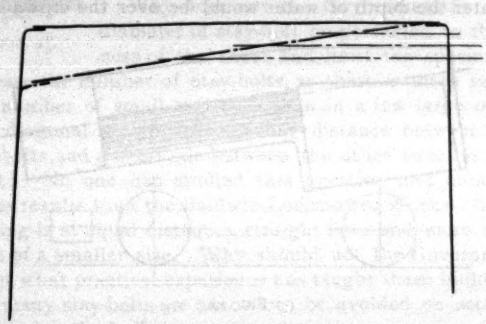

FIG. 19.

Now, this boiler was placed that way in this boat, and the propeller came out at the stern like that, and the engines, I believe, were in this region, immediately aft of the boilers. Now, the crown-sheet was level and the boat, according to the testimony which I listened to, was making a speed of somewhere between fifteen and eighteen miles an hour. If it had been figured in accordance with the elapsed time, as the testimony gave, it would be eighteen miles per hour. The builder claims that he doesn't think the boat can make eighteen miles an hour; but anywhere from fifteen to eighteen miles an hour. If the boat—I won't give you the nautical term, because I don't know it; but if the boat was

lying level—that is, if the boiler was level with the water, when it was standing still—when the boat would be driven at speed and the water line would change into that (exaggerating it), the bow coming out of the water and the stern being pulled down by the screws, the water line in the boiler would appear like that (*Fig. 19*), wherever it might come; and consequently the tendency would be very much greater to have a scarcity of water on the crown-sheet when the boat would be running at maximum speed if the engineer was watching the gage in the engine-room than it would had the boiler been turned around the other way in the boat and operated. Then the faster the speed the greater the depth of water would be over the crown-sheet.

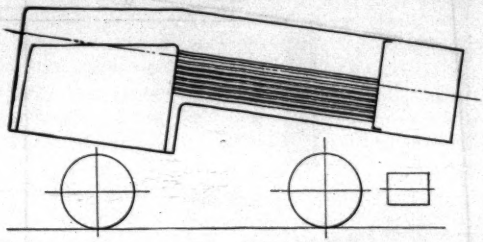

FIG. 20.

In locomotive practice it is customary to lower the crown-sheet at the back three inches (*Fig. 20*) so that if you do get in an abnormal position you won't drop one portion of the crown-sheet, but when you get at that angle the water will be practically level all over the crown; and when we build for abnormal grades running from 10 per cent. to 25 per cent., the boiler, in addition to having a very much angled crown-sheet, is also placed on the locomotive in this fashion; so that when it gets on grade the boiler is comparatively level and in proper working condition.*

*It may be of interest to add that, since the meeting, Mr. Hartman has reported to the editor that in a trial made on a similar boat, no difference in the position of the water-line could be detected when standing still and under full speed.—[ED.]

MR. HARTMAN:—The remark made that "Life is too short to get up intricate stay-bolts," brings up the question whether it is not better to make intricate stay-bolts and save life.

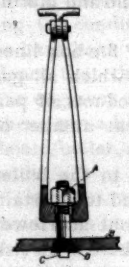

FIG. 21.

The stay-bolt, *Fig. 21*, shown in drawing, is one that can be tightened up without calking, is flexible sideways and can be adjusted to exact length. It has the advantage of the thread and a shoulder in the crown-sheet and the strain is brought on the thread and taper at the same time so that one cannot give way before the other.

Why does not the Government fix on a diameter of stay-bolt proportioned to thickness of the sheet and limit the spacing to increase the number of stay-bolts, as there is more safety in a number of small stay-bolts than in a few large ones? The diagonal spacing gives a long distance between two stay-bolts and a short one between the other two. Is this right? No one has studied this question and obtained better results than the Baldwin Locomotive Works. Their spacing is at equal distances, straight lines and more stay-bolts of a smaller size. Why should not the Government adopt what practical experience has taught these builders? Too many stay-bolts are an evil to be avoided on account of cleaning the boiler.

There appears to be a dread of putting plenty of manholes and handholes in a boiler for cleaning. When they are made amply strong and properly fitted up, there is no trouble with them. To all this comes the reply, you are making your boiler cost too much. But the fact is, that, comparing the cost of the explosions that occur from defective construction all through our land and the additional cost of building boilers properly, it will be found that the proper construction is by far the cheapest in the end. The use of limestone water should be prohibited unless the lime is precipitated before entering the boiler, which can now be done. Why boil lime mud, waste coal and ruin the boiler?

In marine boilers the pure distilled water from surface

condensing is used, which keeps the boilers clean, and no risk is run of burning from heavy deposits of scale. Why not use surface-condensers exclusively with land and marine boilers and get the highest efficiency?

An ideal boiler would have a cylindrical fire-box lined with fire-brick, kept hot by the firing, and in which all gas shall be burned to carbonic acid, and the products of perfect combustion then passed through a great number of small tubes using condensed water.

Having no direct radiation from the fire to the boiler or tubes, the heating surface must be increased to a certain extent, for we have a larger volume of heat but at a lower temperature, which requires the increase. There is no risk of burning the boiler. On the other hand we have a boiler of the highest type of efficiency with a long life. The bricks lining the fire-box are prevented from melting by the surrounding water, while the intense heat insures perfect combustion before the gas reaches the tubes. Once unconsumed gas reaches the tubes, the heat is absorbed by the tubes and no further combustion takes place, the escaping gas goes up the chimney in great black clouds, causing a loss and poisoning the atmosphere.

Many years ago, there was built at the works of I. P. Morris & Co., a boiler with the inside and outside shell bolted together by stout flanges at top and bottom. The outside shell could be removed, leaving the interior free for the workman to get at for repairs and cleaning.

MR. GEORGE B. HARTLEY:—I would ask Mr. Vauclain if any testimony was given regarding the working of the feed-pump or the feed-pipe. I was not in the city at the time and do not know. Now I assume in this argument that the pump was working as it ordinarily did; that the valves on both boilers were opened—one perhaps a little more than the other; because one boiler will take more water than the other when both are feeding from one line; and in this instance I understand that the engineer opened this one valve wide. Was there any evidence offered as to why that was necessary, or why the boiler did not feed as it had been right along? Or was anything said about the

condition of feed-pumps or feed-pipes; whether they were found to be free from obstructions?

MR. VAUCLAIN:—There was nothing in evidence regarding the condition of the feed-pumps. They were supposed to have been working all right—at least, there was no evidence that they were not; and the reason, I believe, that the engineer assigned for opening this feed-valve wide was there was a little less water in this boiler than in the starboard boiler, and for that reason he opened the feed-valve wide.

MR. COLVIN:—I understood that this discussion was to have been on boilers generally, and not particularly on the exploded structure. I have had some little experience in the boiler business. I would like to ask if any gentleman here ever knew a crown-sheet—had positive proof of one—coming down, unless it was for want of water? I have known them to carry 200 pounds of steam on the engine. I never knew one to go down unless it was for the want of water, unless the sheet would become heated so that it got soft in other places. I have had thirty-five to forty years of experience, and I have seen a good many crown-sheets down, and I have never seen one that gave out unless it was, evidently, as plain as day, from want of water. Just to demonstrate that: The crown-sheet bolts are generally larger; more precaution is taken with them than the side stays in the same locomotive; generally they are $\frac{3}{4}$ inch when the crown is only $\frac{7}{8}$ inch. You do not hear of the stay-bolts pulling out the side sheets at all—that is out of the question; it is always on top and in the place indicated by Mr. Vauclain.

MR. LE VAN:—I would like Mr. Vauclain to tell me why these threads were stripped off the bolts at the back end of the boiler and also at the sheet. If that crown sheet was hot it would not have stripped off. The ends of those stay-bolts were just as though they were turned off in the lathe.

MR. VAUCLAIN:—In answering Mr. Le Van's question I simply want to remind the gentlemen present that I have already taken the trouble to state that I believe that that was the hottest portion of the crown-sheet; and as the flame

passed over this portion of the crown-sheet it became heated, and that this portion and this portion particularly were not affected by the heat to the same extent. I also stated that my opinion was that the water had just about reached the top of the crown-sheet, or not quite, and that this heat had driven what water might have been there away, and it was barely possible that these sheets were protected to a certain extent at their extreme ends; and the condition of the studs and the condition of the steam, both at the front and back, and the condition of the plate also, indicated that such probably had been the case; therefore, the tendency to pull these threads and shear them off—not as though they were taken off in a lathe, but as though pulled off the grain of the iron—was a natural consequence; these in front, not so much so; and a careful examination of the boiler will show that that was the case; whereas, in the center or immediately forward thereof, the studs were more or less sloughed off, as though the metal had been quite hot. One of these studs taken out about this point and cut and etched and a portion placed under the microscope would indicate that the bolt had been hot on the end, or that it had been heated on that particular end hotter than it had on the portion of the bolt that was up here. This end that had protruded through and which had sloughed off indicated that the temperature had been quite high, so far as we were able to determine.

MR. LE VAN:—I would like to state that at the point which you stated was the hottest the threads were not stripped; it was next to the flue sheet where they stripped, and I cannot understand how you can make any distinction between the temperature at the flue-sheet and at the front of the box. How do you arrive at that?

MR. VAUCLAIN:—I think that it would be difficult to explain this matter more clearly. Without entering into personalities, it is difficult to make a man see who will not see; but if a bridge-wall is placed in any vessel the portion immediately over the fire, or where the fire starts over the bridge-wall, if the blaze is severe, is much hotter than the part over here, or than the part that is back there; and if

there is any disposition to doubt that, let us take an ordinary cook-stove (*Fig. 22*). Here is the fire and here is the bridge-wall, and the flames are passing over. In this case the flame is drawn straight, goes under this plate (the flame is not drawn down immediately and passed through the tubes here), so that the action here on the back plate will be much more severe than it would in the case of this boiler; but any man here who has ever been a boy and has watched his mother's cook-stove, will remember that when the plates commenced to get hot this one got hot first, and usually the plate began to redden on this portion and not on the front portion. Nobody ever saw the front of a stove burn out quickly; it will last quite a long while; while the bearing

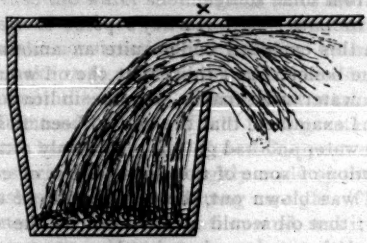

FIG. 22.

plate in the center was always the first to go; that the bearing plate in the center which carried the plates (and as you will remember it carried across like that, and the two plates were hung to it); this plate would sink in the center and in some were supported. So, therefore, if we go back to our childhood days and remember what we saw then, we can easily understand why this crown-sheet was hotter immediately over the bridge-wall, or back of it, than it was at the back end or at the front end.

MR. GEO. B. HARTLEY:—I would say if there is a marine-boiler manufacturer here he can testify that crown-sheets of marine boilers frequently come down, and not necessarily because of low water. They have come down so that in a 36-inch or a 40-inch corrugated furnace you can scarcely get

a 5-inch space between the down part of the crown and the grate-bars—remained, in fact, almost flat down on the grate-bars. Possibly that is due to oil; and I have been waiting for some one to suggest the possibility of oil in this boiler. When examining the "Quaker City" boilers, the engineer climbed in side first, and before I got in I asked him where the oil came from that was on his hands (it was thick). He replied, "While shaking the braces," showing that there was considerable oil in the boiler. I said, "I think you had better boil this boiler out with soda before you get under way again." He said, "We have done it;" showing that there must have been an excessive amount of oil in the beginning, or that the oil was going in there all the time from some source.

MR. VAUCLAIN:—Mr. Hartley's point is well taken. I noticed in this exploded boiler quite an amount of oil had been in the boiler; but I think that the oil would probably make the water very foamy, and the indications were, in the boiler I examined, that the oil had been floating on the top of the water and had gotten very firmly attached to the upper portion of some of the stays. Now, of course, when the boiler was blown out, there was a film of oil on top of the water; that oil would settle down on the crown-sheet and around the stays and make them appear very badly; but it is a fact (it is not a practice that I approve of) that a great many people are feeding oil into their boilers in order to keep them from making scale, and having trouble from that cause.

MR. GEO. B. HARTLEY:—I would like to show another condition that existed in the "Quaker City" boilers. Into the back head of the boiler, on a line about four inches above the crown-sheet, was a gage cock, four inches above this was a second gage cock. These cocks, when tried, would show when the necessary amount of water was in the boiler. In addition to these, and connected from the side of the boiler at each end, was a water column. On to these columns were attached in the regular way three gage cocks and a water glass gage. The columns were connected to the boiler by the usual top and bottom pipes (top for steam, bottom for water.)

This water column I believe to be imperfect, the column proper being of so small a diameter that the trying of a gage cock would lift or siphon the water out, even though the water would be several inches below it, thus misleading the one who tried the gage as to the true water level. The glass gage would show the true level, providing the connections to it were clear, but in the "Quaker City" I found the glass broken and the connections to it closed. A water column should have a sectional area of at least nine square inches with 1¼-inch pipe connections thereto.

THE CHAIRMAN:—I have no doubt that our friends who build the water-tube boilers would say, "build the boilers so that the vital parts will not be exposed to such extreme danger in case the water should get a little low."

MR. FALKENAU:—I would like to ask Mr. Vauclain whether in the construction of his boiler he has any suggestion to make as to what could have been done to avoid to some extent the danger of structural weakness in the "City of Trenton" boiler. Would not a greater amount of riveting have been advisable? From all appearances and the calculations which I have roughly made of the stay-bolts, the bolts were in themselves strong enough. The strain, I believe, amounted to about six thousand, was it not?

MR. VAUCLAIN:—Between six and seven thousand.

MR. FALKENAU:—Do you think there would have been any decided advantage in having had much larger heads, or having put nuts on the ends of the stays?

MR. VAUCLAIN:—Replying to the question, I beg to state that there is quite a difference of opinion on that point. So far as the stays in a crown-sheet are concerned—in a side sheet which is practically the same as the crown-sheet, excepting that the water very seldom if ever gets away from the side sheet, no one ever dreams of having any further protection than merely riveting the stay-bolt end over; and the stay-bolt end is riveted over very slightly; it is considered that the less height the rivet head has, the less trouble they will have with that stay-bolt leaking in the fire-box itself. As the sheet is there, and if the stay-bolt simply projects ⅛-inch and is hammered over, it is

considered better than if the stay-bolt had a head that was $\frac{1}{4}$ inch or $\frac{5}{16}$ inch high; because in the latter the head would become hot and expand the thread or pull in the sheet, and then when the bolt would cool off and the fire would be drawn, the belt would contract and the water would ooze out, which is a condition that prevails in a great many locomotive boilers—especially in bad-water districts.

I was in Indiana quite recently in the shops of the Vandalia-Terre Haute Railroad, and I was informed that it was a daily practice to get in with the hammer and poen up these stay-bolts; that the water was so bad that the tendency of these bolts to heat and expand and then contract and get loose was very annoying indeed. In a crown-sheet we simply reverse the condition of affairs; but what the average railroad man fears is that some expert of a locomotive engineer will let the crown-sheet get hot—will not carry enough water; consequently he resorts to some precaution, and that precaution is generally a device of his own. One man will tell you that he doesn't want anything under his crown-stays—that when a crown gets hot he wants it to let go; but he builds his crown tapering; that is, he lowers it at the back so that when it gets low in water it will commence to be exposed in this manner, and these bolts will let go and that will fulfil the office of the fusible plugs; and he will get in with the jack and reset his sheet. Others suggest the putting on of a nut, and others object to it. Others put in a button head. Others don't like the button head, because it pulls off too easily if it gets hot (I never saw one pull off yet) and they put a hex head on. I might tell you of a number of other devices that are used in order to keep this crown-sheet from coming off when it gets hot; but invariably it *comes* off if the plate has been left to get hot enough.

Answering fully the question that was asked me, we are viewing this boiler not as we would have viewed it if we had been called upon to examine it—if we had been called in before there was any accident; but we now have our hindsight, and we can think of a whole lot of things that would have kept up that crown a little longer; and among

the number of things we might have put on a nut; we might have put more stays in, and put them closer together; we might have done a great many things. If I had been the designer, and would have had the knowledge that I have now of locomotive boilers, I would not have used any of this rigging at all; I would have used an entirely different method of supporting the crown-sheet—perhaps a more slender method. There are some two tons of material, I think, in this boiler that might have been saved; but that is neither here nor there; I think that it would be very ungentlemanly in me, if not a complete violation of professional ethics, to criticise the design of this boiler; because I would be criticising these things after the accident had happened and not when the boiler was designed and when it was put in service. The Government furnishes men paid salaries for this particular thing; and this boiler was designed by the engineer, I suppose, at Neafie & Levy's; and the testimony was that it was submitted to the Government—their inspectors—and the "Board sat upon it." I don't know what that means, but suppose they looked at it and they approved it; they made their calculations, probably, and they approved it; and it was built and accepted and put in service; and I feel perfectly safe in saying that if the crown-sheet had not been overheated at that point it would have been running yet.

THE CHAIRMAN:—We are now ready for further discussion.

MR. FULLERTON:—Mr. Vauclain says, if he should design that boiler now, with his present knowledge, he would use a lighter construction there. I think he would make it interesting to us if he would kindly suggest how he would have supported that crown-sheet.

MR. VAUCLAIN:—In the first place, the gentleman did not state the matter quite right. I said that if I had designed this boiler with the knowledge that I have of locomotive boiler construction I would not have designed it in this way at all, not because this accident happened and with the result that has been chronicled. If I had been designing this boiler—I will only treat the fire-box (as

that is the important part) and the holding of the crown-sheet which let go. Now this boiler had a flat crown-sheet, if I remember correctly, and was so shaped (*Fig. 23*); it had crow-feet in the top here, and studs in the bottom; and a link with a hole in there and a hole in there, suspended on the crown-sheet. Any one who has worked at the business at all knows the difficulty of getting every one of those a tight fit. I suppose it can be done; but life is too precious to waste time in doing that; therefore, if you will permit me, I will draw my half on this side. I would have made

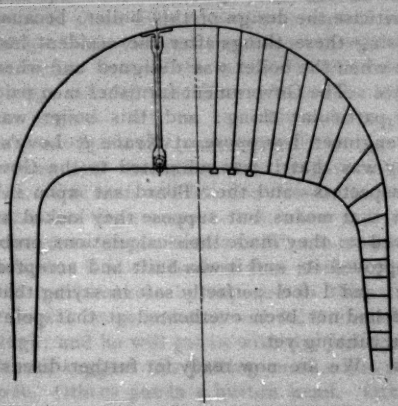

FIG. 23.

FIG. 24.

my crown-sheet like that (indicating), and in place of this system of links I would have simply started radial with this crown-sheet and put in a system of ordinary screw-stays which I will draw in full for you (draws), making these 1 inch in diameter and reducing these to $\frac{7}{8}$ inch or a little less between the threads, where they enter the sheet; and the side-stays from there down, I would have made them $\frac{7}{8}$ inch in diameter at this point and about $1\frac{1}{8}$ inch in the center. They would have been shorter stays. These stay-bolts would have come through there; there would

have been a slight angle to this crown-sheet, about 3 inches, so that from the tipping of the boat, if the boat went down behind, the crown-sheet might probably have been level. The sheet in this case was ½ inch thick all round. I would not have made it over ⅜ inch; and if the crown-sheet had been made separate from the side sheets, so that they could have been removed easily in case of blistering or mud-burning on the side, I would have made the crown ⅜ inch and the side only $\frac{5}{16}$ and screwed the screw-stays into this and have rested my case there. The center rows in the crown-sheet here, if I had known that I was going to use a bridge-wall, would have had nuts or head bolts as a further safeguard to prolong the time that would be required to pull the hot sheet over the end of that bolt. [Applause.]

MR. FALKENAU:—I would like to ask Mr. Vauclain for what object he would round the crown-sheet in his form? That is, as I recall it, in the Bellepaire construction you have a flat crown-sheet, practically, stayed vertically; whereas, in this of that you have a sort of radial staying. What advantage you deem that there?

MR. VAUCLAIN:—In a Bellepaire the top is usually flat and we have a square corner which couples onto the round of the boiler here; the stays come vertically through the sheet, and we get a good thread through each sheet. Then we start with the side sheet, and then we have transverse stays to stay each portion of the sheet; and those are usually heavy on account of getting in a sufficient number. In this case the top of the boiler is rounding, and in order to get a good thread in the thin crown-sheet, that is, to have the thread at right angles to the crown-sheet, it is necessary to have these stays radiate, as near as possible, from the center from which this curve is struck; and as this curve changes where it gets smaller and smaller, of course the pitch on the outside is much larger. If you would take the other side of the boiler and have it just as it is now, in order to get threads through there at right angles, we would have to put a stay that way, a stay that way; and when we come here we would have our threads—we would not have a full

thread in the sheet. Here, we always have a couple of full threads in the sheet. In this case we would not, and we would have a large surface there exposed to the pressure;

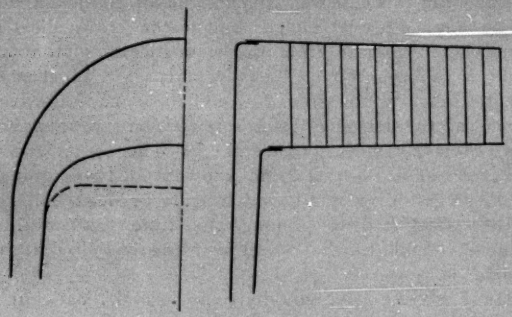

FIG. 25.

secondly, it would necessitate some more stays across in this direction in order to stay this surface. With this construction, however, that is avoided and those stays are not necessary, for the reason that in any crown-sheet, whether it be round or flat, there is just as much tendency for this sheet to go up as there is for that sheet to go down; there is an internal pressure tending to pull these two sheets apart; and if the bolt gets loose, the sheet gets soft and the bolt loses its holding power, the plate bulges down and comes down.

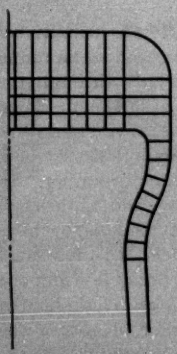

FIG. 26.

Now, it is further desirable to reduce the number of stays as much as possible; because the less stays we have in the boiler the less weight we have and the more room we have for water. There has not been a single mention made of the scarcity of water in this boiler. As designed, the space over the crown-sheet had a great deal of iron in it; but no one

ever asked how much water there was in it, and the more iron you have in over your crown-sheet the more difficult it is for an engineer to feed his engine, so that he has a uniform depth of water over his crown-sheet; or, in other words, if a space over your crown-sheet is taken up with iron very largely and there is a limited amount of room for water, your boiler will make or lose water much more rapidly than if held with a system of staying, such as this; and that is what has made this a preferable staying over any other style, and we have them all.

MR. HARTMAN:—In regard to boiler inspection, I ask: Is that (*Fig. 10*) the proper thing to put in a crown-sheet? You never know where the thread is going to begin on the crown-sheet or when it is going to begin on the screw of the lug. On screwing it in the crown to fetch up fore and aft, you will find it will often fetch up on the shoulder athwartship. What is the boilermaker going to do? He simply sticks a spud in the hole and yanks her around fore and aft. There is then a strain on the thread anywhere from 300 to 1,300 pounds. These lugs $1\frac{1}{4} \times 3\frac{1}{2}$ inches cover too much of the crown and keep the water from protecting the threads on the lug. I have seen boilermakers kink a stay-bolt if it was a little long, to shorten the distance between the ends; but on applying pressure the stay bolt straightens out, leaving some other part take the strain and do double duty. In other cases have seen boilermakers poen the stay-bolts cold to make them longer, which weakens the bolt. Since soft steel has come into use, in recent years, it has proved so serviceable that it is with us to stay. Should not the Government in its formula for spacing take into account what is the stiffness of the plate? I think that has been lost sight of.

Should we not have adjustable stay-bolts that can be made just the right length to suit circumstances? Then heating your boiler as hot as you can at atmospheric pressure, the internal strains from riveting will adjust themselves; you can adjust the stay-bolts to get a more uniform tension of all parts.

Should not the Government fix the diameter and depth of all cold rivet heads on any part of the boiler?

Should not the inspector after supplying his 50 per cent. excess of pressure in testing go into the boiler to see if any stay-bolts have been strained or started? I have no doubt but that it was this excess that started some of the rivet heads of the "Trenton's" boiler.

THE CHAIRMAN:—The subject is still open for discussion.

MR. KINNEY:—When Mr. Hartman was giving the account of the experiment he made there, he made no mention of having made a comparison of the two kinds of metals: the metal he used in his experiment and the metal out of which those stays were made—whether both of the same kind, possessing the same ductility and the same tensile strength. There is quite a difference in his statement as to its resistance value and that of Mr. Vauclain, who stated 60,000 pounds; and Mr. Hartman gave it as the result of a practical, actual pull, as only 32,150 pounds. Now there is quite a difference.

MR. HARTMAN:—That is, steel as near the composition of the steel in that crown-sheet as you can get. The lugs were made of iron, as near as we could judge, and of the same quality that those old ones were. We split one of them in two by sawing it down part way and broke it open to examine the grade; and we tried to put the thing as nearly under the same conditions as possible. I tried to find who furnished that plate, to get a piece from them; but the steelmakers wouldn't tell me; so we had to fall back then on a piece of steel that was as near the same quality as possible, but on riveting our sample found our iron riveted better than the original stay-bolt lugs.

————:—Why can't the gentleman make one test, or a series of tests, of those stay-bolts?

THE CHAIRMAN:—He asks, if there was one or more tests—one test, as I understand Mr. Hartman?

MR. HARTMAN:—We made one test that was pulled slowly, giving it about 25 minutes; the other test we pulled quick. Both of them commenced to spall the rivet at the same strain, but pulling the other plate so much quicker it

ran higher; but giving 25 minutes, the rivet was badly broken up at 28,000 pounds, and final rupture was at 32,150 pounds.

THE CHAIRMAN:—I merely wish to make one observation in few words in reply to my friend Hartman, more especially in relation to the subject of the steel plate, namely: he speaks about the question whether the Government should not use another formula for the resistance of flat surfaces to buckling, or to giving way under pressure, when we deal with soft steel. I think there is probably some misunderstanding on this, from the fact that in speaking of very low tension steel, which we know is very ductile—we can bend it over inside or outside without any show of breaking—I was going to say here that the very exhaustive tests, not only one, but the results of hundreds and many hundreds of tests, I think very clearly demonstrate that the stiffness of the metal, that is, its resistance to pressure or to yielding under pressure by buckling, shows that it follows very closely its tensile strength; in other words, if that steel was, say, as low as 50,000 pounds tensile strength and you were comparing it with iron of 50,000 pounds, that the resistance would be very similar. If 60,000 pounds, it would be about one-fifth more.

MR. HARTMAN:—In reference to the heat in the fire-box it strikes me that there are two phases to that, from what experience I have had in other boilers. Calling that the bridge-wall (*Fig. 17*), when you first throw your coal on the fire your carbonic oxide is generated so rapidly that it is not all burned near the front—this is comparatively cool; but when it gets further back, then the air that strikes it here from back of the bridge-wall you will find gives the greatest heat there; that is, when you put fresh coal on. Now when the coal has burned and got up good and bright, then you get your most intense heat over the bridge-wall. Why not use a fusible plug at each place?

For some three or four months—during the time that the work on the "Trenton" was being completed—Mr. Wilson was confined at home with a serious illness; but at the time it was being finished he got down to the works on three

occasions with the help of his nurse to look around at the work, so that personally he could not give it the inspection that he would have done if he had been well.

THE CHAIRMAN:—We will adjourn for this evening, thanking you all for your attendance.

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## ELECTROLYTIC LEAD AND OXIDES.

At the recent meeting in Philadelphia of the recently organized American Electrochemical Society, Mr. Pedro G. Salom read a paper describing a process of his invention for electrolytic lead reduction which is in operation at Niagara Falls. The principle lies in the use of lead sulphide as cathode in an acid solution, the hydrogen cation forming with the sulphur gaseous hydrogen sulphide, and thus reducing the lead sulphide to lead in spongy form. The form and construction of the cells used were described and the difficulties discussed which had been encountered. The principal difficulty was that the reduction was not complete, and the degree to which the ores are reduced varies without apparent reasons. It was stated that at present 92 to 95 per cent. of the sulphide is reduced. Another difficulty is that the workmen's eyes are affected in the operation of the process. Two pounds of lead are produced per horse-power hour, 48 cells in series being electrolyzed at 130 volts. Specimens were shown of spongy lead, compressed sponge, litharge and other materials made by Mr. Salom's company. Owing to the spongy form, the material is very readily transformed into litharge. The company intends to take up later the manufacture of storage battery plates.

W.

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## SAPPHIRES IN NORTH CAROLINA.

One of the most valuable of the recent contributions to the literature of the sapphire is that of Dr. Joseph Hyde Pratt, mineralogist of North Carolina, published as Bulletin 180, Series 9, of the United States Geological Survey. On page 9 of the paper he discusses two or three localities where it occurs in gneisses and schists. In Cowee township, Macon County, it is found in seams or layers in a hornblende (amphibole) gneiss, which is itself derived from the alteration of an igneous rock (gabbro), the augite having been changed into hornblende, and the rock much sheared by pressure. The corundum shows plainly that it was an original constituent and not an alteration product. The other occurrence is even more interesting, as it shows the presence of corundum in gneisses and quartz-schists derived from sedimentary beds. These rocks extend along the crest of the Blue Ridge from Rabun County, Ga., to Clay County, N. C., and carry bands or zones of corundiferous schist, conforming to and belonging with the rest. All these schists, Dr. Pratt regards as very ancient sandstones and shales, greatly elevated, eroded and metamorphosed; in the course of which changes the aluminous shales yielded first bauxite, and then the excess of alumina crystallized as corundum.—*Engineering and Mining Journal.*

# PHYSICAL SECTION.

*Stated Meeting, held Wednesday, November 27, 1901.*

## THE QUESTION OF THE DIVISIBILITY OF THE ATOM.

By A. STANLEY MACKENZIE, PH.D.,
Member of the Institute.

There has never been a time since the atomic theory of matter first took its place in the world of science as a tried and tested hypothesis that its reasonableness or sufficiency has not been questioned. It is true that the objection of many people to the theory has been based on *a priori* reasoning, and not on the capability or incapability of the theory to co-ordinate experimental evidence. Indeed, the fact is too frequently lost sight of that this is one of the prime objects of a theory, if not the only logical object. The fundamental meaning of a theory is an imagined mechanical model that, with its supposed motions, characteristic properties, degrees of freedom, etc., will simulate and reproduce for us, when subjected to the laws of dynamics as we know them for ordinary matter, the behaviors of unknown agencies. The atomic theory has always met with much objection, because it forces us, we are told, to believe in the existence of absolutely hard, round, perfectly elastic small bodies, or else of bodiless centers of force, or of ghostly pieces of electricity getting their apparent mass in an equally ghostly way, or else of infinitesimal cyclones of ether differentiated from the omnipresent ether only in that they are in vortical motion, that is, that they are in suspension in a continuous fluid of their own material, and only on account of their motion are singled out for our comprehension. We are told that the mind cannot admit the existence of such things as realities of the same nature as the bodies we can lay our hands on. In this kind of objection we are confounding two quite different conceptions. In the first and most important instance the atomic theory is a mechanical model—a mathematical model, if you

like; in the second place, it is an effort to specify the ultimate physical constituents of matter. We do believe that in so far as our researches have carried us we have in matter a coarse-grained structure; but we must admit that we may have misnamed the inherent or essential nature of the atom; we may have called it a little, hard, round piece of matter (with most of the properties we associate with molar bodies) when it was a piece of ether; or we may have called it ether when it was electricity, etc.; but in every case the same analytical treatment would apply, that is, we should retain practically the same theory with an altered nomenclature. The test of a good theory is not its beauty or symmetry, nor its *a priori* likelihood, but its accuracy as a model and its suggestiveness. By this test the atomic theory is a good one, and all advance in our knowledge concerning the behavior of matter as such has been made under its guidance. But the special claim it has to goodness is that nothing has been advanced thus far which will take its place.

It has, however, been mainly on other grounds that criticism, not captious, has been directed against the atomic theory. It is that it has not gone far enough; we are left with seventy-odd kinds of atoms, with no suggestion for an explanation of their difference, with no link to establish their connection; and the thought has been almost an universal one during the whole past century that this connection must exist, and that the number of different kinds of atoms can be reduced and some relationships between them established. Even Dalton makes the following admission: "We do not know that any one of the bodies denominated elementary is absolutely indecomposable." As early as 1816, while the atomic theory was still in the hypothesis stage, Prout made his famous deduction from the rather inaccurate numbers then known for the atomic weights of the elements, that these are exact multiples of that of hydrogen, and that consequently all the other atoms are compounds of the hydrogen atom. It is safe to say that the result of the work done in relative atomic weight determination since the time of Prout has been to make these

numbers on the whole to approximate more and more to whole numbers. It is true that the number for chlorine is almost exactly 35½; but notwithstanding that, the closeness of many of the numbers to multiples of that of hydrogen is remarkable. The Hon. R. J. Strutt has determined recently the chance that the relative atomic weights should approximate so closely as they do to whole numbers, supposing their values to have been determined in some haphazard manner. He found it to be as small as 1 in 1,000; so that, as he remarks, we have stronger reason for believing in the truth of some modification of Prout's law than in that of many historical events which are universally accepted as unquestionable.

If there is one thing more than another that makes us look with hopefulness for a proof of some such relationship as Prout's, it is the aid it should give toward the introduction of a theory of gravitation. There is no natural phenomenon more universal and less understood than the non-selective action of gravitation, and it is one to which attention is seldom called in text-books on physics; they are apt to leave beginners with the impression that gravity *should* act on all bodies alike, but magnetism differently. This fact early suggested to Graham that all the different elements may have the same atom or ultimate particle, but existing in different combinations of movement. Lavoisier, again, was careful to state that he called certain things elements only because with the means at his disposal they were indivisible. Their chemical investigations had led such men as Kopp, Dumas, Berthelot and others to consider seriously the possibility of the compositeness of the atom as the only explanation of certain anomalous phenomena which they had noted.

In 1887 Lockyer published a book entitled "The Chemistry of the Sun," in which he summed up the results of a long series of his investigations in the domain of the spectrum analysis of heavenly bodies. Certain variations in the intensity, characteristics and circumstances surrounding the appearance of spectral lines led him to the belief that the so-called elements could be reduced to

simpler forms, and that these simpler forms actually existed under the enormously high temperatures of the sun and some of the stars. This had been suggested by Brodie and Hunt, years before, from a study of the simplification which matter underwent from elevation of temperature, even of those temperatures we can control and produce on the earth.

No one of these various conjectures took definite form, or led to a specific formulation of an extension of the atomic theory. Such a step has lately come, however, and it arose from the unearthing of a set of phenomena which have been observed for only a scant dozen or so of years, and the important results from which have come upon us with startling suddenness and taken us completely by surprise. They promise not only to reveal to us a relationship of the elements to each other, but to reduce all the elements to compounds of a common stuff; indeed, to tell us the actual weight and properties of this *ur*-atom, this fundamental piece of matter out of which all the known different kinds of matter are made. This hypothesis goes far beyond Prout's wild guess; it not only reduces the number of elements, it cuts the atom—the "uncuttable." It is a magnificent generalization, and most fertile in the possibilities it holds forth, if it can be put on an established footing.

In order to understand the genesis of this new hypothesis, it is necessary to give a rapid summary of the experimental evidence accumulated along these lines during the last few years.

In 1859 Plücker noticed the phosphorescence on the walls of a vacuous glass tube opposite the cathode when an electric discharge is passing through the tube. It seemed as if something emanated from the cathode and traveled away in straight lines, and Plücker found that these cathode "rays" are deflected by a magnet. Goldstein, in 1876, found that these rays started off normally from the cathode, and he concluded that they were waves in the ether. During the years 1879 to 1885 Crookes made a profound study of these rays and considered that we had here to deal with the motion of negatively electrified particles

of matter projected with enormous velocities normally from the electrode, causing fluorescence, phosphorescence and heat by their impact on the walls of the vessel and objects placed in the tube, and mechanical motion by impinging on lightly mounted vanes, etc. He called this indeed a fourth state of matter. The two views, (1) that the rays are disturbances of the ether, and (2) that they are electrified particles of matter, have each had its school of supporters. The fact that the rays are deflected in a magnetic field points at once to the latter explanation and has presented an unanswerable argument to the ether-theory believers. J. J. Thomson and others have calculated from the amount of this magnetic deflection the velocity of the motion of the rays; it is about $3 \times 10^9$ centimeters per second. Thomson has found also that the radiation is not homogeneous; that a part of it is not deflected at all, and produces no phosphorescence. He has, moreover, found that for the same potential difference between the electrodes the magnetic deflection is the same in all gases. It was found also that when the cathode rays fall upon an obstacle or constriction in the tube the rays are diffusely reflected, and that the far side of the obstacle acts itself as a cathode. When cathode rays, therefore, fall upon a surface it becomes a cathode to all intents and purposes.

Closely connected with these rays are the so-called "canal rays," studied by Goldstein in 1898. They come from the yellow layer next the cathode and can be observed best when they stream out backward through a perforated cathode. They travel in straight lines, but produce no luminescence and are not deviated in a magnetic field. They have a velocity of about $3.6 \times 10^7$ centimeters per second, and the ratio of the mass of a particle, $m$, to the charge it carries, $e$, is about $3.3 \times 10^{-4}$, which is in accordance (as we shall see later) with the supposition that it is ordinary atoms which are projected and at work in this phenomenon. It seems probable that they are part of the return stream of particles to the cathode.

The early experimenters with cathode rays found that even very thin bodies cast black shadows; but in 1892

Hertz discovered that gold and aluminium foils transmitted some of the rays, and concluded from this that the latter could not be particles of matter, and that they must be ether waves, perhaps of the nature of ultraviolet light.

In 1887 Hertz had found that when ultraviolet light fell on a body the rays would discharge it if negatively charged, but would have no effect (or according to Branly a very feeble effect) on a positive charge. If the body is uncharged, the illumination by ultraviolet light gives it a charge which is almost always positive. The air between two condenser plates becomes a conductor when ultraviolet light falls on one or both of them, and there is a leakage current across the dielectric. It is evident that the gas plays a large part in the discharge, but this discharge is also dependent upon the metal of which the plates are formed. Since that time a corresponding effect has been found with X-rays, but it is there entirely produced by the gas and is quite independent of the material of which the plates are made.

At about the same time it was observed that gases could be electrified and made conducting by being brought near hot metals, and that electrified bodies in the gas would be discharged. The gases in all these cases behave as if they had become ionized; and this supposition has been most fruitful as an explanation of the phenomena involved.

Important advances now came with great rapidity. In 1894 Lenard made the cathode rays pass out of the vacuous vessel in which they were produced, by putting a very thin aluminium window in the vessel opposite the cathode. He found a diffuse light spreading out from the window to a short distance, 5 centimeters, into the outer air. These rays seem to possess all the properties of the cathode rays inside, but to distinguish them they are called "Lenard rays." They cause bodies to phosphoresce; they affect photographic plates; they discharge positively or negatively charged bodies; they make the air conducting; etc. By admitting the rays that pass through the window, not to the outer air, but to another enclosed space, the pressure and kind of gas in which could be regulated, Lenard found that the absorption of the rays was constant, no matter what

was the nature or pressure of the gas, provided the density was always the same. This is a matter of prime importance from a theoretical standpoint, for it makes the absorptive power depend only on the density of the gas and not on its chemical composition or its physical state. Lenard observed, also, that these Lenard rays are deflected by a magnet in exactly the same way as are the cathode rays, and that they have a velocity about one-third that of light. The deflection is independent of the nature and the pressure of the gas through which they pass. Lenard still inclined towards the ether theory of these radiations. Most of the upholders of this theory have held that the vibrations of the ether were transversal, but Jaumann suggested that they might be longitudinal. The chief support of an ether theory is the fact that the rays can pass through substances and even out of the tube into the open air, and the chief difficulty the theory has met with is in accounting for the action of a magnet. The electrified particle theory accounts for this action at once. In 1895 Perrin performed some experiments which make for the particle theory. He caught the cathode rays in a sort of Faraday ice-pail and found that they charged the vessel upon which they struck with negative electricity, and J. J. Thomson has proved that the rays carry their charges with them when deflected by a magnet. These experiments brought out another most important fact, namely, that the rays make the gas through which they pass, a conductor for the time being. Thomson suggested that they break the gas up into some kind of ions, and this explained to him why he had failed to find a deflection of the rays in an electrostatic field, as he had expected if they were electrified particles; for, since they move in a conducting field, they would be no more deflected than a current in a wire. Later, Thomson was able to get rid of these ions by using higher vacua, and he then found that the rays were deflected in an electrostatic field. The deflection under these circumstances, combined with the magnetic deflection, leaves us practically with no alternative but to believe that the rays are electrified matter in some form.

In 1900 Lenard showed that the impact of ultraviolet

light on a conductor placed in a vacuum caused the latter to give out rays charged negatively and deviable by a magnet, like the cathode rays.

In 1895, the year after the appearance of Lenard's work on the Lenard rays, came another important discovery, that of Röntgen of the rays called after him, or simply "X-rays." He found that when the electric discharge is passing through a vacuous tube covered with black paper, which is opaque to visible, ultraviolet, cathode and Lenard rays, a radiation is emitted by the tube through the black paper, which acts on photographic plates or a fluorescent screen at a great distance. These rays are accordingly much more penetrative than cathode rays; in fact, all bodies are more or less transparent to this radiation, and roughly in the inverse ratio of their densities. They are absorbed to a certain extent by air and other gases. They discharge both positively and negatively electrified bodies when they fall upon them, whether the bodies be conductors or insulators. They ionize the gas through which they pass and make it conducting. They are not deflected by a magnet, and hence are not cathode rays. They come most strongly from the phosphorescent patches on the wall of the tube made by the cathode rays. They do not possess the properties of ultraviolet rays of being regularly reflected, refracted and polarized. When they fall obliquely on a sheet of metal the other side of the metal gives off radiations which affect a photographic plate, but which have not exactly the properties of the original rays. Whenever the X-rays strike a metal surface there is emitted by the surface secondary radiations consisting of two parts: one not deviable in a magnetic field and in many ways similar to the primary radiation, and another part deviable, and consisting of rays agreeing in their properties with cathode rays. These secondary rays have notably a less penetrative power than the primary rays, but they still ionize a gas and discharge electrified bodies. Röntgen at first supposed the X-rays to be longitudinal vibrations of the ether. These radiations are not homogeneous; that is, they are not all equally

absorbed by a given substance. Though they ionize the gas through which they pass, as do ultraviolet light and cathode rays, yet they differ from the ultraviolet rays in that the latter ionize a gas only after being reflected from a fluorescent substance or a metal plate, etc. Air loses its power to conduct after passing through water or cotton wool; this fact and the small value of the velocity of the ions of a conducting gas make it seem probable that the ion is the center of an aggregation of a considerable number of molecules.

In 1897 Stokes suggested a very plausible explanation of the X-rays. Assuming that the cathode rays are moving negatively electrified particles, surrounded, in consequence, by a magnetic field of force, when the rays strike suddenly on an obstacle there will be electromagnetic induction, due to the stoppage of the quasi-current, and a thin pulse of magnetic and electric force will be propagated through the medium; it is this pulse in the ether which constitutes the X-rays. J. J. Thomson and Lehmann have independently, at about the same time, given the same explanation. It can be shown that such pulses would be of the same nature as ordinary light in many ways, but, being pulses, would differ from such regular trains of waves as constitute light in having less absorption by matter, and would have no regular period and no regular reflection and refraction and no diffraction. This simple explanation is now very generally accepted, and we have the X-rays put in their proper place as ether waves, along with infra-red, visible and ultraviolet rays, and consequently in an entirely different category from the cathode and Lenard rays, which are streams of negatively electrified particles.

In 1896 Becquerel discovered that uranium and its salts emit (even after being kept in the dark for years) radiations which have a great many properties in common with ultraviolet rays, with cathode rays and with X-rays, and yet the uranium rays cannot be identified as belonging to any one of these three groups. They travel in straight lines; they affect photographic plates and fluorescent bodies; they discharge electrified bodies when they fall upon them. In

these respects they behave exactly as do all the three groups of radiations referred to above. Like the cathode and X-rays, they ionize a gas through which they pass and make it conducting, and Rutherford has shown that the velocity of the ions is the same for a gas made conducting by uranium radiations as for one made so by X-rays. The cathode, Röntgen and uranium rays all act as nuclei for water condensation in a dust-free atmosphere. The uranium radiations do not show any regular reflection, refraction or polarization, at least not the most active rays, and in this respect are again like cathode and Röntgen rays, and unlike ultraviolet radiation. Metallic uranium will not hold a charge unless in a vacuum, thus acting as if ultraviolet light fell upon it. The uranium radiation is not homogeneous, but is composed of rays of different penetrability. Rutherford has made a detailed study of these properties, and divides the uranium radiations into two groups: the one, $\alpha$, more intense and very easily absorbed; the other, $\beta$, more feeble and very penetrating.

Soon it was found that thorium and its compounds have the same general radio-active properties as uranium, but the radiations are more penetrating. In addition to the "rays," Rutherford has shown that the thorium compounds give off an "emanation" or gas, which is probably made up of fine material particles, themselves radio-active, and which has the power of making bodies on which it falls radio-active. M. and Mme. Curie have isolated from pitch-blende two new substances, polonium (seemingly allied to bismuth) and radium (allied to barium), with a radio-activity 100,000 times as great as that of uranium; the polonium radiations are at the same time very little penetrative. From the same kind of material Debierne has made actinium (allied to thorium). Still more active substances of this kind are now prepared by De Haën, of Hanover. The radiations from all such substances are grouped under the generic title of "Becquerel rays." The radiation of radium is composed of three parts: (1) rays not deviated in a magnetic or electrostatic field and not very penetrative; (2) a small amount of non-deviable rays which

are very penetrative, and (3) deviable rays which are very penetrative. The latter give the same value for $\frac{m}{e}$ as do cathode rays, and indeed have all the properties of cathode rays, including the carrying of a charge of negative electricity, and are indistinguishable from them except in the method of production. The same is generally true of all Becquerel rays; they are, therefore, very like the secondary X-rays. Any substance placed near a radio-active body acquires itself a radio-activity, the so-called "induced radio-activity," which may persist even for days at a time. At a very high temperature Rutherford has shown that the radio-active properties of these substances is destroyed. MacLennan has lately imparted radio-active properties to many substances by letting cathode rays impinge on them for half an hour, and by letting the light from a spark-discharge fall on them, and then gently heating them; but similar experiments made with ultraviolet rays and X-rays have failed.

Zeeman, in 1896, noticed that the bright lines in the spectrum of a source of light placed between the poles of an electromagnet were widened when the field was turned on. The same effect is produced in the absorption line spectrum of a vapor when the latter is placed in a strong magnetic field. An explanation of this phenomenon was furnished by the electromagnetic theory of Lorentz, which assumes that in all bodies small electrified particles, or ions, or "elektrons," are present with a definite mass and charge, and that all light phenomena are due to motions of these ions. These ions when moving in a magnetic field experience mechanical forces which influence their periods. Lorentz's theory suggested at once that the edges of the widened lines should be polarized, and on looking into the matter Zeeman found this to be so. Lorentz's theory explained further that when viewed along the lines of magnetic force a single line was broadened because it has become really a doublet with its two components circularly polarized in opposite directions, and when viewed at right angles to the magnetic force the broadening was due to the

fact that the single line has become a triplet with the middle ray plane polarized parallel to the lines of magnetic force and the extreme rays plane polarized perpendicular to those lines. The theory also required that the distance between the extreme rays in either case should be dependent upon the ratio of the mass of an ion to its charge, and that $\frac{m}{e}$ should be equal to

$$\frac{H(\lambda_1-\lambda)}{4\pi V},$$

where $H$ is the intensity of the magnetic field, $V$ the velocity of light, and $\lambda_1$ and $\lambda$ the wave-lengths of the extreme rays. Experiment has verified these predictions of theory. It further appeared that it was the effect of the magnetic force on the negative ions which produced the phenomena.

Further work by Zeeman, Cornu, Preston, Larmor, Michelson and others has shown that the phenomenon is much more complicated than indicated above, and that a line may be broken up into more components than three, but the elektron theory still accounts for these additional peculiarities when it is viewed in greater detail; but the main features for our purpose are those already mentioned.

Such is the enormous amount of experimental material gathered together by investigators in this small department of science in a short time; the bare recital of their results has taken considerable time. We are now in a position to see how the evidence presented by this great mass of experiments has been able to shed light on the nature of the ultimate structure of matter.

When dealing with the cathode rays and allied phenomena we saw that there were two theories to correlate and account for the observed facts—an ether theory and a corpuscular theory. Either one will account for the rays starting normally to the cathode and traveling in straight lines, for the heating effects and for phosphorescence, and for the purely mechanical effects. The deflection of the rays by magnetic and by electrostatic forces seems explicable on the corpuscular theory only; but their passage through a thin-walled tube is so readily explained on the ether

theory that it has given great support to such a theory and for a time caused considerable trouble to the adherents of the corpuscular theory. J. J. Thomson has given some attention to this point. He points out first that the rays may not really pass through the wall, but only start afresh on the other side. He shows that the motion of the particles shot off from the outside of the window in the Lenard experiment would be of the nature of an impulse, and the momentum gained would be proportional to this impulse; and as the deflection of the rays by a magnet is proportional to their momentum, we should expect to find that deflection independent of the nature and pressure of the gas outside the tube; and this is exactly what Lenard noticed. But inside the tube also the magnetic deflection is independent of the nature and pressure of the gas, and yet the motion there is not impulsive and the masses of the molecules of the different gases vary greatly. This led Thomson to the question, what are the corpuscles or particles which carry the charges? The first light thrown on this matter is the observation by Lenard that the rays outside the tube in air at a pressure of half an atmosphere travel only 5 millimeters before their intensity is halved. In the light of the kinetic theory of gases we can deduce from this that the mass of a corpuscle cannot be that of a molecule of air. Moreover, it cannot be that of an aggregate of molecules, because Lenard observed that the distance traversed by the rays is inversely proportional to the density of the gas. This leaves only the possibility that the corpuscles are of smaller mass than ordinary molecules and atoms. Thomson was satisfied that this suggestion would account for the observed phenomena; for if an atom is made up of a great number of holes and corpuscles, the holes being predominant, the mean free path of a corpuscle would be inversely proportional to the number of corpuscles it would meet, that is, to the number of corpuscles in unit of volume, that is, to the mass in unit of volume, or the density. This is exactly what Lenard found, if we assume that the penetrating distance of the corpuscles is their mean free path. Moreover, if all atoms are composed of the same corpuscles, whatever

the gas, the magnetic deflections would be the same for all substances, as was found by Lenard to be the case.

This was conjecture. What was now wanted was experiments which would determine numerically the mass of the carriers of the negative electricity. One experiment which we have already referred to gave some evidence in this direction. The amount of the widening of a line in the Zeeman phenomenon gave a measure of $\frac{m}{e}$, and Zeeman found it to be about $10^{-7}$ C. G. S., E. M. units. The values of $\frac{m}{e}$ for ordinary electrolytes are well known; that for hydrogen being about $10^{-4}$ units. J. J. Thomson and others now directed their energies to finding the value of $\frac{m}{e}$ and of $e$, for ionized gases; the product of the two values would give $m$. Thomson's first method was to let cathode rays fall on a thermopile for a given time and thus measure their energy, $W$; after they pass the thermopile they are caught in a vessel connected with a capacity and an electrometer and the total charge, $Q$, on them measured; at the same time the radius of curvature, $\rho$, of the bending of the rays in a magnetic field of strength, $H$, is measured. Then

$$\frac{m}{e} = \frac{1}{2}\frac{QH^2\rho^2}{W}.$$

The value of $\frac{m}{e}$ thus got was $1{\cdot}5 \times 10^{-7}$, and was independent of the gas and of the nature of the electrodes.

In a second experiment he measured the deflections produced by a magnetic field, $H$, and by an electrostatic field, $F$, when each acted over a distance, $l$, and so arranged the forces that the deflections, $\theta$, were equal. Then

$$\frac{m}{e} = \frac{H^2 l}{F\theta}.$$

The value for $\frac{m}{e}$ thus found was $1{\cdot}3 \times 10^{-7}$. Similar measurements by Lenard and Kaufmann gave similar results.

In his third method Thomson investigated the phenomenon noticed by Elster and Geitel of a decrease in the conduction between two plates illuminated by ultraviolet light when subjected to a magnetic force normal to the lines of flow between the plates. If the strength of magnetic field, $H$, reduces the flow to a minimum when the distance of the plates apart is $d$, then

$$\frac{m}{e} = \frac{H^2 d^2}{2V},$$

where $V$ is the velocity of light. The result for $\frac{m}{e}$ was $1{\cdot}4 \times 10^{-7}$.

Here are four entirely different ways of getting at the value of $\frac{m}{e}$, and they all give the same result, namely, that $\frac{m}{e}$ is about $10^{-7}$, and that it is independent of the kind of matter employed. The same value has been found by Becquerel in the case of the deviable radium radiation.

We have thus the values of $\frac{m}{e}$ for ordinary electrolytes and for ionized gases, and the latter is about $\frac{1}{1000}$ of the former for the case of hydrogen. It remains to find the relative values of $e$ for hydrogen and for the ion of an ionized gas, and we can deduce the relative values of $m$ for the two cases. C. T. R. Wilson discovered that the ions produced by ultraviolet light act like those produced by X-rays in providing nuclei in dust-free air around which water vapor can condense in small drops. If then we could note the total charge carried by a collection of these drops when they fall, and at the same time count the number of them, we would have the charge on each. Moreover, if we can find the size of the drops we can find their number. The size of the drops can be determined from their rate of fall as a cloud. In this fashion Thomson found the value of $e$ to be $6{\cdot}5 \times 10^{-10}$ C. G. S., E. S. units. From a knowledge of the mass of hydrogen required to carry a unit of electricity, combined with a use of the kinetic theory of gases, it is proved that the value of $e$ for hydrogen is of the same order

of magnitude as that which was found for the ion. Townsend has proved this fact by an entirely different method, namely, by the rate at which ions diffuse into gases. Since, therefore, the $e$'s are the same for the two cases, and the $\frac{m}{e}$ for the corpuscle is $\frac{1}{1000}$ of the $\frac{m}{e}$ for hydrogen in a liquid electrolyte, it follows that the mass of a corpuscle is about $\frac{1}{1000}$ of that of a hydrogen atom, and that it is the same for all gases. The mass of this negatively charged corpuscle in a gas at low pressure is about $3 \times 10^{-28}$ of a gramme. It is found, however, not only in low-pressure vacuum tubes, but it is given off by incandescent metals, by metals illuminated by ultraviolet light, and by radio-active substances; and in every case it has the same mass and the same charge, and a velocity which is a large fraction of that of light, the radium corpuscles having the greatest velocity, namely, two-thirds that of light. Thomson states that with the appliances of ordinary magnitude the quantity of such matter produced is infinitesimally small; he calculated that his machine going night and day for a year would produce only $\frac{1}{1000000}$ of a gramme. This at once explains why the chemistry of matter in this state is not studied.

The carriers of the positive electricity behave quite differently from those of the negative, for it is found that the same quantity, $e$, of electricity, instead of being associated with a constant mass equal to $\frac{1}{1000}$ of the mass of an ordinary hydrogen atom, is connected with a variable mass, namely, one of the order of magnitude of that of a molecule of the substance under consideration, thus varying with the nature of the gas in which the electrification is found.

We have then in following the negative charge been led to a something of an invariable mass, of very small amount as compared with the masses of the molecules of ordinary matter, and independent of the kind of matter with which we found it associated—a veritable *urstoff*. What is this corpuscle? Is it a piece broken off from the ordinary atom and at the same time splitting up the neutralized electricity on that atom and taking the negative charge with it and

leaving the positive on the remainder of the atom, and are all the elements made up of the same corpuscles, but differing from one another in number and arrangement? And have we the right to say that we have accordingly found the connecting link between the elements? This is one view, and it seems a very plausible one. Or was it electricity only that was split off from the atom? But it has mass, and how are we to conceive of electricity as endowed with mass and apart from matter with our present views? It is true that if we have a corpuscle of matter moving with a charge on it, it has an apparent added mass due to the magnetic field it bears with it; but is it reasonable to dissolve away the carrier of the electricity and say that the latter still has mass? These questions are still to be answered. J. J. Thomson has tried some experiments to test whether the mass is to be associated with electricity alone or whether part of it is due to a carrier, but his investigations have not led him to any definite conclusion.

The results of the experiments briefly described above and the new conceptions based on them have been of value in other fields; they can be used to account for electrical and thermal conduction in metals; they have even led to a very plausible explanation of the Mendelejeff groups and the aurora borealis. But we must confine ourselves here to their bearing on the structure of matter. The difficulty to be overcome in applying them to this problem lies in the question of the seat of the mass, and whether we have in the ion, or elektron, or corpuscle, a thing of the nature of an atom, only smaller, or something else. The matter seems to be mainly a question of words. We have found a thing with a mass $\frac{1}{1000}$ of that of a hydrogen atom, using throughout our calculations the term mass in the only way we can mechanically use it. Now we know matter only as a thing having mass, and we only use the idea of mass in mechanics as a property of matter; hence we know atoms and molecules only as they possess mass; so if mass is our criterion, we have in the elektron a sub-atom, and since it is the same for all elements, we have by inference the ordinary 70-odd atoms as aggregations of this sub-atom. If it should turn

out to be true that the energy is purely electrical, it would not materially alter our atomic theory, it would give us simply an electrical "explanation" of mass—a concept for which at present we have no explanation.

PHYSICAL LABORATORY,
BRYN MAWR COLLEGE.

## NOTES AND COMMENTS.

### "PECULIARITIES OF THE WRITING OF HEALTHY PERSONS."*

BY AUGUST DIEHL,
Assistant Physician, Clinic for the Insane, Ruprecht—Karl University, Heidelberg (now Nervenarzt, Lübeck).

p. 4 xx "It appears desirable after *the initial step into the domain of the scientific study of writing*,† first to establish a firm basis for the comprehension of the writing of those sound in health in order to avoid the danger of errors and misunderstandings in the realm of the diseased."

p. 4. The persons whose writing was studied were four male and four female attendants at the University clinic (Ruprecht-Karl.)

p. 5. The questions to which answers were sought were (1) whether and what regular relations exist between the various peculiarities of handwriting. (2) How the chirography is influenced by slow and fast writing. (3) What changes it exhibits when the task is more difficult. (4) How far the individualities of handwriting may be regarded as personal peculiarities and indicate sex.

Ingenious devices together with the kymographion were used to measure the length of the path pursued by the pen, and the time consumed.

The hospital attendants employed as subjects were of the educated peasant class.

The experiments were undertaken in two series of five days each. The writings were performed with a Kohinoor H.B. pencil kept properly sharpened, and the substance written on was well-sized cards. The time chosen was 9.30 A.M.

The tasks assigned were as follows:

(1) Please write slowly and carefully the numbers 1 to 10 on the card.
After this had been done:
(2) The same thing once more.
After a pause of two minutes.
(3) Now, please, write the numbers 1 to 10 as fast as you can.
When this was finished:
(4) Once more, please, as fast as possible.

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*Translated Extracts from Doktor Dissertation.

†This initial step was taken in 1894 by the translator in his book "Bibliotica."

The last task was :

(5) Please write the row of numbers reversed from 10 to 1 just as it is most convenient for you.

The time was called . . . . . Sd.
The length of path of the pen . . . . . Sw.
The time in hundredths of a second for a millimeter of path . . Mz.

Then $Mz = \frac{Sd}{Sw}$ if Sw be expressed in mm. and Sw and Sd in $\zeta$ (= 0·01 seconds.) The greatest variations between the means of the writers was about $\frac{1}{10}$ second for a single letter. The average time taken was nearly $\frac{1}{10}$ second to a mm.

The conclusions are : (1) The pen-path is shortened by increased and lengthened by decreased difficulty of the task. (2) The rapidity is lessened by writing backwards and increased by force of will and excitement. (3) The pen-pressure increases with voluntary effort, and decreases by diminished difficulty of the task. (4) The pauses in writing are influenced even in a higher degree by the alteration of the task set than by the rapidity. (5) The rapidity of writing single figures depends most of all on the pen-path, then on the intermediate pauses. But besides this there exists with the effort to write with uniform rapidity a certain tendency to approximate to each other the periods of writing different figures—long figures fast, short figures slowly. Finally the placing of a figure in the row may change the rapidity under the changing influences of the will, of practice, of habit, and of fatigue. (6) Practice appears to make the chirography smaller without increasing its rapidity; the pressure usually diminishes through its influence. (7) With unfavorable mood we find retardation of writing together with increase of pressure, also sometimes diminution of the written characters. (8) Pen-path, rapidity, and pen-pressure are in a high degree characteristics of a single individual; they appear to alter in nearly the same relation by change of the experimental conditions in different men. The same cannot be said of the rapidity. (9) The capacities of different persons for writing are only comparable when undertaken under identically the same conditions. (10) Women write larger, more quickly, and with less pressure than men; the writing movement is made by women with greater ease and less effort of will. (11) On increasing the difficulty of a writing task men respond preferably by raising the effort of the will, women by a diminution of size of the written characters. P. F.

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## CAUSE OF AUTUMNAL HAZE.

The Chief of the United States Weather Bureau offers the following explanation of the phenomenon :—The dry haze is undoubtedly due to fine particles of dust. The finest dust is composed of one or all of the following substances, namely, fine particles of soil or the dead leaves of plants, smoke or ashes from wood fires, salt from ocean spray, the shells or scales of microscopic silicious diatoms, germs of fungi, spores of ferns, pollen of flowers, etc. In the still air of the damp nights these dust particles settle slowly down, and the morning air is comparatively clear. During the daylight the sun warms the soil, which heats the adjacent air, and the rising air currents carry up the dust as high as

they go. Under certain conditions, which are named in the letter, the layer of dust reaches higher and higher every successive day. During long, dry summers in India it reaches to 7,000 feet with a well-defined upper surface that is higher in the day-time than at night-time. This is a general explanation of dry haze weather and applies to Indian summer also. The reason why we have more of such weather in the autumn is because there is then less horizontal wind and less rising air. W.

## BOOK NOTICES.

*American Standard Specifications for Steel.* By Albert Ladd Colby, Member of Committee No. 1, of the American Section of the International Association for Testing Materials. Second edition. Easton, Pa.: The Chemical Publishing Co., 1902. (Price, $1.10.)

Mr. Colby gives a review of the work of Committee No. 1 of the American Section of the International Association for Testing Materials, showing how their proposed specifications were arrived at from the specifications in use. He refers to the general discussion, both at home and abroad, of these proposed specifications during the past two years, and their final adoption by the American Section last summer.

A general review is then given of the requirements of the specifications, with the reasons which governed the Committee in its decisions, and takes up each of the following headings:

(1) Process of Manufacture.
(2) Chemical Properties.
(3) Physical Properties.
(4) Test Pieces and Methods of Testing.
(5) Finish and Variation in Weight.
(6) Branding.
(7) Inspection.

After this there is a review of each of the nine specifications. These reviews are excellent and give a clear, concise statement of the conditions governing the work. The specifications are becoming the recognized standards of this country; the Rail Specifications, with some modifications, having recently been adopted by the American Railway Engineers and Maintenance of Way Association as their standard.

The book is a good addition to our engineering literature on the subjects treated, and its price is only moderate. W. R. W.

*Annuaire pour l'an 1902*, publié par le Bureau des Longitudes. Avec les notices scientifiques. Paris: Gauthier-Villars, 1901. (Price, 1 fr. 50c.)

The publishers of this annual call special attention to the contribution of M. A. Cornu on Polyphase Currents, and that of M. H. Poincaré, on Wireless Telegraphy. The book gives all the statistical data that have made the previous volumes so valuable for reference. W.

*Theory of Steel Concrete Arches and of Vaulted Structures.* By William Cain, Professor of Mathematics, University of North Carolina. Second edition, thoroughly revised. 16mo, boards, pp. 161. New York: D. Van Nostrand Co., 1902. (Price, 50 cents.)

This volume is a revised edition of the author's previous volume on "Voussoir Arches, etc.," which appeared as No. 42 in the *Van Nostrand Science Series.*

The author states that he has undertaken in this to give a complete solution of the elastic arch of variable section, and a detailed consideration of the arch of steel and concrete combined to illustrate the general graphical treatment.

About one-half of the present volume is stated to be new, and, taken as a whole, while it is independent of the author's other works on arches, practically supplements them.

The work treats, in chapter I, of arches of variable section under vertical loads; in chapter II, of culverts and tunnel-arches; in chapter III, of grooved and cloistered arches; and in chapter IV, of masonry domes. W.

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*Jahrbuch für das Eisenhüttenwesen.* Ergänzung zu "Stahl und Eisen." Ein Bericht über die Fortschritte auf allen Gebieten des Eisenhüttenwesens im Jahre 1900. Im Auftrage des "Vereins deutscher Eisenhüttenleute" bearbeitet von Otto Vogel. I. Jahrgang. (Preis elegant gebunden in Ganzleinen, 10 M.) Verlag von August Bagel, Düsseldorf.

This new Yearbook of Iron Manufacture is intended to supplement the well-known journal, "*Stahl und Eisen*" and the "*Gemeinfasslichen Darstellung des Eisenhüttenwesens,*" issued by the "*Verein deutscher Eisenhüttenleute.*" It is intended that the present publication shall record in systematic order the numerous contributions to the literature of the metallurgy of iron at home and abroad, by the publication of abstracts of the most important of those contributions by which they will be made more readily accessible. The present first volume of the Yearbook forms a large quarto of 450 pages, and contains about 1,800 references and abstracts, comprising the useful literature in this field to be found in 110 technical journals and other publications, and in eight languages. The whole is thoroughly indexed by authors and subject-matter. W.

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# Franklin Institute.

[*Proceedings of the Stated Meeting held Wednesday, May 21, 1902.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, May 21, 1902.

President JOHN BIRKINBINE in the chair.

Present, 68 members and visitors.

Elections to membership since last report, 11.

The Actuary reported that the Board of Managers had passed the following resolution at its stated meeting of Wednesday, May 14th, viz.:

"*Resolved*, That a special committee be appointed to act in conjunction with a similar committee to be appointed by the Institute to prepare a memorial of Dr. Henry Morton, deceased, for publication in the *Journal*."

It was thereupon voted to appoint a special committee for the purpose.

The Chairman named the following members: Dr. Coleman Sellers, Chairman; Washington Jones, Samuel Sartain (from the Board), A. E. Outerbridge, Jr., H. R. Heyl and Cyrus Chambers, Jr. (from the Institute).

Dr. Joseph W. Richards, of Lehigh University, presented the conclusion of his remarks on "The Electro-Chemical Industries at Niagara Falls." His present remarks bore specially on the Acker electrolytic sodium process; Hall's electrothermic method of purifying beauxite; the Ampere Electro-Chemical Company's method of producing electrothermic barium hydrate, cyanides and nitric acid, and Acheson's procedure for the electrothermic production of graphite.

Mr. C. H. Ott, Assistant Engineer, Bureau of Surveys, Philadelphia, presented a communication on "The Improvement of the Channels of the Delaware and Schuylkill Rivers by the City of Philadelphia." (Discussed by Prof. Lewis M. Haupt.) The paper was fully illustrated with lantern views.

Under the title of "A Floating Palace," Mr. W. N. Jennings exhibited and commented on a series of lantern views of the yacht "Margarita," owned by Mr. A. J. Drexel.

Mr. Chas. E. Ronaldson was elected to the Committee on Science and the Arts, to take the place of Mr. R. D. Kinney, withdrawn.

Amendments to the By-laws were offered by Prof. F. L. Garrison, providing for dropping from the list the names of delinquent members; and by Prof. L. F. Rondinella, providing for a letter-ballot at the annual election. These amendments are to be voted on at the stated meeting of June 18th. The Secretary was directed to give members due notice of the amendments by publication.

Mr. Jesse Pawling, Jr., Secretary of the Physical Section, reported the action of the Section at its stated meeting in April, appointing a special committee consisting of Messrs. Jesse Pawling, Jr., Chairman; Dr. A. E. Kennelly and Dr. George F. Stradling, to bring to the attention of other societies and of individuals the action of the Institute in reference to the Metric System of Weights and Measures, and to ascertain from them their views on the report adopted by the Institute.

Mr. Pawling called attention to the fact that the three members of this special committee were also members of the Committee on the Metric System (appointed by the President at the stated meeting of November 20, 1901), and suggested that it would be more appropriate for the Institute to confirm this action of the Physical Section, and thus authorize the proposed correspondence to be carried on by a committee of the Institute. This suggestion was approved. Adjourned.

WM. H. WAHL,
*Secretary.*

## COMMITTEE ON SCIENCE AND THE ARTS.

(*Abstract of proceedings of the stated meeting held Wednesday, May 7, 1902.*)

MR. JAMES CHRISTIE, *Chairman pro tem.*

The following reports were adopted:

(No. 2209.) *Synchronism Indicator.*—Paul M. Lincoln, Niagara Falls, N. Y.

ABSTRACT.—This instrument was devised for the purpose of synchronizing two or more alternating current generators which it is desired to connect in parallel, and indicates the phase and frequency relation of two separate sources of electromotive forces. The instrument also indicates which of the generators is operating at the higher frequency and what the difference is.

The invention is protected by U. S. letters-patent No. 685,155, October 22, 1901, granted to applicant.

A full description of the device may be consulted in the JOURNAL (vide Lincoln, Parallel Operation of Alternators, Vol. cliii, 241, April, 1902), to which reference is made.

The report finds that the instrument fully meets all of the requirements of practice, and greatly facilitates the connecting of alternating current machines in parallel. It is pronounced to be a meritorious and ingenious invention. The award of the Scott Legacy Premium and Medal is recommended to the inventor. [*Sub-Committee.*—Wm. C. L. Eglin, Chairman; Coleman Sellers, Thos. Spencer, W. M. Stine.]

(No. 2225.) *Improved Kerosene Oil Incandescent Light.*—Williams, Brown & Earle, Philadelphia.

ABSTRACT.—The subject matter of this application is an adaptation of the kerosene oil incandescent light for use in the magic lantern and for various uses in photography.

The device is based on the system generally known as the Kitson light, but is specially modified in respect of portability and compactness, so as to adapt it for the above-named applications.

In respect of intensity of illumination this light is intermediate between that produced by the best multiple-wick oil lamps and a good oxy-hydrogen light. The illumination is superior to that of any oil flame, being far more even and white. These qualities adapt it well for various kinds of photographic work.

The Edward Longstreth Medal of Merit is awarded to the manufacturers. [*Sub-Committee.*—F. E. Ives, Chairman; L. E. Levy, W. N. Jennings.]

(No. 2184.) *Machine for Cutting and Otherwise Preparing Rattan.*—Julian Pomeroy and Henry W. Larsson, Springfield, Mass.

(An advisory report.)

Under-mentioned reports passed first reading:

(No. 2212.) *Rheostat.* Charles Wirt, Philadelphia.

(No. 2228.) *Device for Photo-micrography.*—Fred'k Eugene Ives, Philadelphia.

Protest was filed by Messrs. Francis Head, A. M. Greene, Jr., and Arthur J. Rowland against report on the Toerring Enclosed Arc Lamp, adopted at the adjourned meeting of April 11, 1902.

WM. H. WAHL, *Secretary.*

## SECTIONS.

(*Abstracts of Proceedings.*)

PHYSICAL SECTION.—*Stated Meeting*, held Wednesday, April 23d. Dr. Stradling in the chair.

The first paper of the evening was read by Mr. J. Frank Meyer, of the Lower Merion High School, Ardmore, Pa., on "Recent Work in Electric Convection." The reading of the paper was followed by some appropriate remarks by Dr. Stradling.

The second paper was read by Prof. Richard Zeckwer, on "An Investigation of Piano Touch." The paper was discussed by Dr. Stradling, Mr. Meyer, Dr. Partridge and Dr. Goldsmith. A number of interesting lantern slides were shown in connection with the paper.

The committee appointed at the last meeting to bring before the various scientific, technical and manufacturing bodies the action of the Institute on the metric system, reported that no action had as yet taken place, owing to the change in the constitution of the Institute, which was contemplated at the time the committee was appointed.

The meeting then adjourned.

JESSE PAWLING, JR., *Secretary.*

CHEMICAL SECTION.—*Stated Meeting*, held Thursday, April 24th. President L. F. Kebler in the chair. Present, 15 members and visitors.

Dr. J. Merritt Mathews presented an informal communication on "Mercerizing Textiles," which was replete with interesting historical and technical data. A number of specimens of mercerized fabrics were exhibited. Discussed by Dr. H. F. Keller, the chairman, and the author.

Mr. Kebler followed with a communication entitled "An Examination of Physical and Chemical Tests for Strychnia." The author called attention to, and demonstrated the possibility of, color reactions being misleading. Discussed by Dr. Robert H. Bradbury and Dr. Keller. A paper, which was to have been read by Dr. E. A. Partridge, on account of the lateness of the hour, was postponed.

M. I. WILBERT,
*Secretary pro tem.*

*Stated Meeting*, held Thursday, May 22d. Dr. W. J. Williams in the chair. Present, 27 members and visitors.

Dr. Edward A. Partridge made an informal communication "On Temperature."

The speaker discussed the several current definitions of temperature that have been proposed by Clark-Maxwell, Kelvin, and others, and indicated in

what respects they were unsatisfactory and open to criticism. He discussed the principles of thermodynamics, illustrating their application in the Carnot Cycle, and demonstrated how by this method the absolute zero is established. Discussed by Messrs. Pawling, Reed, Bradbury, Dodge, Whittaker and the author.

Dr. Robert H. Bradbury followed with a "Review of some of the Recent Literature of the Subject of the Periodic Law." Discussed by Mr. Reed and the author. The speakers received a vote of thanks for their interesting communications.

WM. H. WAHL,
*Secretary pro tem.*

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—*Eighteenth Stated Meeting*, held Thursday, May 1, 8 P.M. Present, 15 members and visitors. Dr. Leffmann in the chair.

Mr. Fred. E. Ives presented a communication describing an ingenious method, of his devising, for "Measuring Objects under the Microscope." The speaker illustrated the adaptation of his device by projecting a photomicrograph on the screen, upon which the dimensions were indicated on a scale.

Dr. Leffmann presented a communication on the use of "Light as a Therapeutic Agent."

Mr. Samuel Sartain made some remarks on a new modification of a pigment printing.

Mr. M. I. Wilbert described another modification now being introduced under the trade name "Ozotype."

An exhibition followed of a series of lantern slides and prints of miscellaneous subjects, contributed respectively by Messrs. Ives, Leffmann, Ridpath and Wilbert. Worthy of special notice among these were several prints of the remains of an ancient aqueduct near Cairo, Egypt.

MARTIN I. WILBERT,
*Secretary.*

MECHANICAL AND ENGINEERING SECTION, ELECTRICAL SECTION.—*Joint Meeting*, held Thursday, May 8th, 8 P.M. Mr. A. M. Greene, Jr., in the chair. Present, 38 members.

The paper of the evening was presented by Mr. W. A. Heywood, Assistant Superintendent Pennsylvania Iron Works Co., on "Modern Corliss Engines for Railway and Lighting Service."

Mr. Heywood illustrated his paper with the aid of a series of diagrams. The discussion which followed was participated in by Messrs. Francis Head, Thomas Spencer, H. F. Colvin and the author. The paper is reserved for publication.

Following this came a discussion on the "Relative Merits of the Binary and the Decimal Systems of Measurement for the Shop and Drawing Room," which was participated in by Messrs. Jesse Pawling, Jr., Wilfred Lewis, Geo. Schumann, L. F. Rondinella, John Haug, H. F. Colvin.

DANIEL EPPELSHEIMER, JR.,
*Secretary.*

MINING AND METALLURGICAL SECTION.—*Joint Meeting*, held Wednesday, May 14th, 8 P.M., with the American Institute of Mining Engineers, at the Manufacturers' Club.

Present, about 300 members and visitors.

President F. Lynwood Garrison in the chair.

The following program was presented:

"The Development of the Bessemer Process for Small Charges." Mr. Bradley Stoughton, Columbia University, New York.

The author traced the history of the Bessemer process for small charges from the original stationary converter of Bessemer in 1856. Next came the Clapp-Griffith Converter of 1881; Walrand, 1884, and, more recently, the converters of Robert and Tropinas.

The main difference in these converters resides in the method of blowing. Walraud and Legeneisel, in 1896, patented a method covering an afterblow with subsequent addition of ferro-silicon.

The "Baby Bessemer Process," as it is called, is now apparently in successful operation here and abroad. The loss in iron is from 17 per cent. to 30 per cent., and it is claimed that a hotter steel is produced than by the open-hearth method, thus making it particularly useful for the manufacture of small castings.

The paper was discussed by Mr. Campbell, who took exception to the statement that the steel is hotter than that produced in the open-hearth method.

Mr. Wilcox, in connection therewith, gave some reminiscences regarding the Clapp-Griffith converter.

This was followed by a paper on the "Relation between Structure and Durability of Steel Rails," by Mr. Robert Job, Chemist to the Philadelphia & Reading Railway Company.

Mr. Job's paper covered a careful investigation of a large number of rails which had failed in service. The samples were submitted to physical, chemical and microscopical examinations. The conclusions were reached that failures are more frequently due to physical defects than to chemical composition, and the rate of wear on rails of identical chemical compositions is slower with a fine-grain than with a coarse-grain structure.

The paper was illustrated by photo-micrographs and rail sections.

Discussed by Messrs. York, Wm. Kent, Martin and Dr. C. B. Dudley.

The following papers were read by title:

"The Metallurgy of Titanium," Augusti Rossi, New York; "The Growth of Pig Iron Production during the Past Thirty Years," Mr. John Birkinbine, Philadelphia; "The Effect of Re-heating upon the Coarse Structure of Overheated Steel," Karl Göranssen, Sweden; "The Oil Fields of Texas," Mr. Robert T. Hill, U. S. Geological Survey.

G. H. CLAMER,
*Secretary.*